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Bilateral Monopoly, Successive Monopoly, and Vertical Integration

By Fritz Machlup and Martha Taber

What are the effects of vertical integration upon output and prices? If an industry has been " vertically fragmented ", that is, if it has consisted of a number of separate firms each operating in only one of the successive stages of production, and if such firms are now combined by " vertical merger ", should one expect that, as a result of the integration, the total physical output of the industry will be increased and the price of the ultimate product reduced?

The affirmative answer to this question has been supported by many economists for a long time; but there have been others who denied the contention and again others who argued the opposite. The different answers were, of course, based on different sets of assumptions. The two major points of emphasis have been cost and market position. Some writers have believed that vertical integration would do little to alter market positions but might substantially alter *costs*. There is the possibility of economies of vertical integration, especially where technology is changing and productive establishments, hitherto spatially separated, are brought together; but there is also the possibility of diseconomies through a loss of specialization. Other writers have believed that vertical integration would do little to alter costs but might substantially alter *market positions*. There is the possibility that monopolistic positions are extended, strengthened or consolidated by the merger; but there is also the possibility that existing monopolistic restrictions are relaxed and monopoly prices reduced as a result of the merger.[1]

In this article we shall not concern ourselves with technology and production cost, but only with changing market structure and pricing practices.

I

VERTICAL FRAGMENTATION AND INTEGRATION CONTRASTED

The view that vertical combination or integration would most probably relax output restrictions and reduce prices to consumers has had a long history. In the fabric of this thought there are three main strands with subtle entanglements between them. They have in common the central conclusion that the existence of separate firms at different stages of an industry that contains monopoly elements may account for less-than-monopoly output and higher-than-monopoly prices.

---

[1] For a discussion placing some emphasis on possible economies of vertical integration, see Adelman (1, pp. 28 ff.). For a skeptical view on such economies, see Stigler (51, pp. 189–90). On the probability of increased monopolistic power, see Edwards (15, pp. 404–10). On the probability of a reduced degree of monopoly, see Spengler (49, p. 347). For references in parentheses, see pp. 118–119.

(1) In a vertically fragmented industry there may be monopolistic market positions in some of the stages of production. Possibly firms in these stages are few, perhaps as a consequence of previous horizontal merger; or geographic market structure has given each of the firms a partial monopoly; or practices have grown up that have eliminated vigorous competition. If such monopolistic conditions exist in two or more successive stages of the industry, problems of bilateral monopoly may arise. It has long been contended that, in bilateral monopoly, independent attempts by the monopolists to maximize their profits will lead to less production and higher prices than would result from a policy of joint profit maximization. Hence vertical integration, securing joint profit maximization in the integrated stages, would yield benefits to the consumers (and, on the usual assumption, bring the economy closer to the " Pareto optimum ").

(2) Where no such conditions of bilateral monopoly have developed in a vertically fragmented industry, it is still very likely that monopolistic elements are prevalent in some of the successive stages. It is one of the characteristics of monopolistic pricing that the seller takes account of the elasticity of demand for his output. A monopolist's marginal revenue is lower than his selling price. The demand curve facing a seller at one stage of production (call him *C*) is derived from the demand curves facing the fabricators one stage later in the process (call them *D*'s). The sum of the marginal revenue-product curves of the *D* firms becomes the average revenue curve for *C*, whose marginal revenue curve will therefore be marginal to the aggregate marginal revenue-product curve of the *D*'s. The demand and marginal revenue curves which enter into *C*'s calculations will, as a rule, be less elastic at equal quantities than those considered by the *D*'s. If *C* sets discriminatory prices for his intermediate product in an independent attempt to maximize his profits, the reduced elasticities of demand at given quantities will cause him to set higher prices and produce a smaller output than would be the case if he combined with the *D* firms. An analogous argument holds for a monopsonistic buyer in any of the successive stages. Vertical integration between stages previously in the hands of independent successive monopolists or monopsonists would avoid the cumulative downgrading of elasticities and would consequently secure larger outputs and lower prices to consumers.

(3) When the conduct of firms, and especially their pricing practice, is based on the full-cost principle, vertical fragmentation of an industry may lead to a piling up of successive mark-ups. If each firm follows the practice of adding a fixed profit margin to its " full cost ", and if this full cost includes the profit margin of its suppliers, there must result a mark-up of mark-ups and an " exaggerated " elevation of

prices. Vertical integration would do away with intermediate mark-ups and would therefore, with given percentage mark-ups, reduce the ultimate prices to consumers; output would accordingly be increased.

These three strands of thought are often interwoven. Implicit in the case of bilateral monopoly are the problems of reduced elasticities of derived curves; and the cumulation of mark-ups will not exist unless the structure of the market is conducive to such pricing. But an analytical separation is possible and desirable, and the history of the analysis presents little difficulties on this score.

II

BILATERAL MONOPOLY: A REVIEW OF THE THEORIES

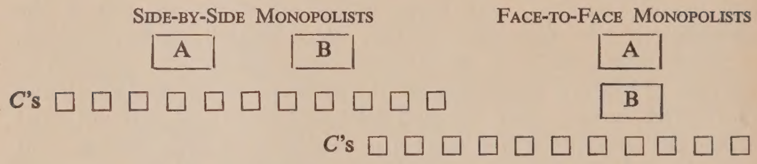
The development of the theory of bilateral monopoly includes some of the greatest names in economics—which makes the temptation of a historical exposition irresistible.[1] A history of this theory has to start with two writers neither of whom used the words or had a clear notion of " bilateral monopoly ". One was Augustin Cournot, who in 1838 analyzed the case of two monopolists producing different commodities " jointly consumed in the production of a composite commodity " (10, p. 99); the other was Carl Menger, who in 1871 discussed the problem of " isolated exchange " or barter between two individuals (34, pp. 175–81).

Cournot had no model of transactions between a sole seller and a sole buyer. But the case of complementary monopolists which he examined—in mathematical terms—has been generally considered analogous to the case of bilateral monopoly. Goods and services needed in successive stages of production are, of course, complementary; and this has led economists to assume, usually without discussion, that there is no significant difference between the complementary and the bilateral relationships. Marshall, for example, mentions Cournot's illustration of the monopolists supplying the copper and zinc needed to make brass, and adds his own illustration of spinners and weavers supplying complementary services in the production of cloth, without examining whether or not the more obviously vertical arrangement in his case makes any essential difference (32, p. 495). Zeuthen was among the few economists who presented formal demonstration of the equivalence of the two cases. Indeed, he used the term " bilateral monopoly " for both.[2]

[1] The historical survey which follows is regrettably incomplete because of its omission, for linguistic reasons, of significant contributions in Italian literature, especially by Amoroso (3, 4 and 5), Fanno (16), and Nardi (35).

[2] " The bilateral monopoly . . . may concern two entrepreneurs supplying raw materials or services who are both sellers to a third, non-monopolistic party (the case of joint demand). It may, on the other hand, concern two monopolistic enterprises standing face to face as buyer and seller. In the main, however, the determination of price is the same in the two cases . . . ". Zeuthen (56, p. 89). See also (56, p. 64, and 57, pp. 1,657 and 1,661).

A diagrammatic scheme of the two cases may be helpful:



Now we can easily see a few differences in the two market structures: the side-by-side monopolists sell to a large group of competing buyers; on the other hand, one of the face-to-face monopolists sells to a monopsonist who is also a monopolist (or what has been called a monemporist[1]). The most important difference, however, is in the extent to which the two monopolists will communicate with each other. The face-to-face monopolists necessarily deal with each other, negotiate and conclude contracts, settle prices and quantities. The side-by-side monopolists need not have any direct contacts with each other, and their " getting together ", if it occurs at all, will be fortuitous or deliberate rather than inevitable. These highly significant differences have recently received increasing attention.[2]

Cournot's particular assumptions permitted a determinate solution for both prices and quantities. Either of the complementary monopolists would take note of the price charged by the other, would assume that his own actions would not affect the other's price, and would then set his price in an attempt to maximize his own profit. His counterpart might now, however, find his own price inappropriate and, again assuming that his own action would not affect the other's price, would adjust his own price with a view to his own profit. A series of such moves would lead to a set of prices which would not induce either of the two complementary monopolists to make further changes. A determinate equilibrium is reached.[3]

[1] Nichol (36, p. 83); Weintraub (53, p. 260).

[2] Another variant is the so-called " mediate bilateral monopoly ", where a monopolist sells to a mass of competitive buyers who are also competitive sellers to a monopsonistic buyer. Thus, the monopolist and the monopsonist face each other over a wall of competitive firms in the intermediate stage. They have no regular business dealings with each other, but are likely to entertain mutually conflicting conjectures and objectives. For a discussion see Stackelberg (50, pp. 33, 41–2). Incidentally, Stackelberg finds that " the position of this market relation is . . . analogous to the case of complementary goods " (50, p. 42).

[3] Readers who know Cournot's theory of duopoly may be struck by the difference in assumptions. In the case of duopolists—competing " monopolists "—Cournot assumed that each in turn would adjust the *quantity* of his output, believing that his counterpart would not alter his quantity. In the case of complementary monopolists, Cournot assumed that each in turn would adjust the *price* of his product, believing that his counterpart would continue to sell at his former price. Edgeworth (14, p. 122), explains this apparent contradiction, observing that Cournot's assumptions follow logically from the concepts of perfect substitutability and perfect complementarity. In a perfect market there can be only one price for perfect substitutes; hence competing monopolists can adjust only quantities. For perfect complements there can be only one quantity (in accordance with fixed coefficients of production); hence complementary monopolists can adjust only the price.

Cournot's equilibrium quantity would be smaller than that which would maximize the joint profits of the two parties; and the sum of the two prices (and the price of the final product) would be higher than the price charged by a combined monopoly. The principle applies also when there are more than two complementary monopolists. Indeed, " the more there are of articles thus related, the higher the price determined by the division of monopolies will be, than that which would result from the fusion or association of the monopolists " (10, pp. 103–4). Their fusion or integration, consequently, would benefit the consumer.

Menger (1871) discussed " isolated exchange " between two individuals trading their goods, each attempting to maximize " subjective values ". Menger found that the " price " would be indeterminate within certain limits, though he added that " as a rule " the price would be equally far from the possible extremes if the two parties were equally strong and skilful bargainers (34, pp. 178–9). His discussion was quite primitive, without the use of either calculus or geometry and without any mention of " bilateral monopoly ".[1] But he proceeded immediately to the case in which one individual as the sole possessor of a good was trading it against another good possessed by several competing individuals; and he did speak of monopoly in this case (34, pp. 181–6). The frame of reference was pure barter; production, division of labour, stages of production, money prices and money incomes were all outside Menger's model.[2]

Edgeworth (1881) presented a remarkable advance in the analysis of both pure barter and bilateral monopoly. He showed that " contract without competition is indeterminate " (12, p. 20); he delineated the range of indeterminateness by deriving " contract curves " from the utility functions of the bargaining parties (12, pp. 20–50); and he discussed the place of intervention or " arbitration " (12, pp. 51–6). Later (1897), Edgeworth criticized Cournot's theory of complementary monopolies as well as his theory of competing monopolies—now called duopoly—and concluded that there would not exist, as a rule, a determinate and stable equilibrium in such situations (14, p. 126). He conceded that a stable equilibrium might be reached in bilateral monopoly " on the extreme supposition that [one of the parties] is perfectly intelligent and foreseeing [while the other] cannot see beyond his nose " (14, p. 125).

Also for Pareto (1896 and 1909) the problems of isolated exchange and of bilateral monopoly were essentially the same. The solution for bilateral monopoly was indeterminate at least within " pure economics ". Using " pure " economic theory, " . . . we arrive at the conclusion that the problem is indeterminate because we do not know what use each of the two would make of his power ". A determinate solution requires additional assumptions " of a more factual type ". Rejecting the models in which two monopolists alternatively establish their preferred prices, Pareto observed that really " there is only one monopolist if the second accepts the price fixed by the first " (39, p. 601).

---

[1] " For bilateral monopoly we have a theoretical prototype, namely, the theory of isolated exchange ", said Schumpeter (48, p. 983), who cited Cesare Bonesana Marchese di Beccaria (1764) as one who anticipated Menger in showing the indeterminateness of the exchange.

[2] Menger, at the time, did not know Cournot's work. See Hayek (22, p. 396).

The first four editions of Marshall's *Principles* made no reference to the problem. In the fifth edition (1907) Marshall included a passage which was, in pleasant contrast to the writing of most of his predecessors and successors, set forth in simple English, without the " aid " of mathematics. He evidently accepted Edgeworth's conclusions that there were no determinate solutions (32, p. 493). Turning to Cournot's example of *A*, supplying copper, and *B*, supplying zinc, Marshall reiterated the view that

> There would then be no means of determining beforehand what amount of brass would be produced, nor therefore the price at which it could be sold. Each would try to get the better of the other in bargaining; and though the issue of the contest would greatly affect the purchasers, they would not be able to influence it.
>
> Under the conditions supposed, *A* could not count on reaping the whole, nor even any share at all of the benefit, from increased sales, that would be got by lowering the price of copper . . . For, if he reduced his price, *B* might raise the price of zinc; thus causing *A* to lose both on price and on amount sold. Each would . . . be tempted to bluff the other; and consumers might find that less brass was put on the market, and that therefore a higher price could be exacted for it, than if a single monopolist owned the whole supplies both of copper and of zinc; for he might see his way to gaining in the long run by a low price which stimulated consumption. But neither *A* nor *B* could reckon on the effects of his own action, unless the two came together and agreed on a common policy: that is unless they make a partial and perhaps temporary fusion of their monopolies. On this ground and because monopolies are likely to disturb allied industries it may reasonably be urged that the public interest generally requires that complementary monopolies should be held in a single hand (32, p. 494).

Marshall added other examples of complementary or bilateral monopoly—a spinner and a weaver, a railway and a sea route. He also expressed an important qualification to the inference that fusion would serve the public interest, but with this we shall deal later.

Pigou (1908) re-emphasized the point that the results of bilateral monopoly " without any agreement between the two monopolists " are " different from those that emerge under a policy of agreements ". Without agreement, " each monopolist takes account of the probable effect of his action upon the conduct of the other, and follows with a counteracting move every move of his opponents ". A continual chain of alterations of price and output would result. With agreement, however, there will be " a less fluctuating condition of industry . . . *Pro tanto* this consideration suggests that a policy of agreements among correlated monopolists is socially advantageous " (40, p. 206). In his further analysis Pigou confined his attention to the case of bilateral monopoly without agreement or combination. In doing so he was probably influenced by his major interest in the theory of wage bargaining between monopolistic unions and monopsonistic employers, where " integration " is clearly not in the picture and " combination " is rare (40, p. 215; see also 41 and 42).

Bowley (1924) first took brief notice of the problem of bilateral monopoly in relation to bargaining between a worker and an employer. In his model, as in some of its predecessors, the employer fixes a wage rate and lets the labourer adjust the quantity of labour supplied. The process of moves and countermoves that follows will not lead to a determinate result except with " collusion " between the two monopolists (7, p. 62).

Wicksell (1925) objected: " If the manufacturer can set the piece rate, then the worker . . . has no monopoly ! " (55, p. 276). For Wicksell the concept " monopoly " evidently implied a certain behaviour pattern[1] and, if such a pattern is assumed to prevail, a determinate equilibrium position will be attained, contrary to the conclusions of Edgeworth and Bowley. Wicksell's exposition is enlivened by a picturesque illustration, drawn from a reference by Babbage (6, pp. 199–201) to the only existing possessor of the skill of making dolls' eyes who sells to the only manufacturer of dolls (55, p. 276). The Wicksellian equilibrium of the bilateral monopoly, as that of Cournot, involves injury to the independently acting monopolists as well as to the consumer, because profits as well as output will be lower than if the monopolists combined or merged (55, p. 271).

Schumpeter (1927 and 1928) held that " Wicksell was not quite fair to Bowley ", but agreed—in contradiction to his own 1908 opinion (45, p. 270)—that " there will exist a determinate equilibrium position " if it is possible to accept Wicksell's assumptions: that both parties know the demand curves, that they pursue only their own individual interests but cannot (and do not try to) force the other " to his knees ", and that any deal is final for the whole period (46, pp. 250–1; 47, p. 371). In these circumstances " the barter point between the parties is perfectly determined, and *not* only the range within which there will be barter . . . Nor can it be held that the assumptions . . . are so very far from reality. They are, if anything, nearer to reality than the assumptions implied in the idea of theoretically perfect competition . . ." (47, p. 371). Schumpeter maintained his position and restated it in his (posthumous) *History* (48, pp. 983–5).

Incited by Wicksell's and Schumpeter's comments, Bowley (1928) elaborated his theory of bilateral monopoly. Reasoning in terms of a monopolistic supplier of ore who deals with a monopsonistic producer of steel, he presents Case I, in which the " manufacturer [of steel] can dictate the price of the material, while the producer of the material decides the output "; Case II, in which " the supplier of the material can dictate its price while the manufacturer decides the output "; and Case III, in which " the manufacturer and supplier of material combine " (8, pp. 653–4). Bowley shows that in Cases I and II the party who establishes the price gains by far the largest share of the profit, while the one who adjusts the quantity receives a meagre monopoly profit; and that the profits of the two together will surely fall short of what could be obtained by combination. Not only would Cases I and II yield indeterminate solutions, but they would be socially disadvantageous because of the lower output and higher price that would result.

---

[1] Stackelberg criticizes Wicksell for taking a definite behaviour pattern as part of the definition of monopoly (50, p. 91).

If, in Case III, the " manufacturer and supplier of material combine to maximize their joint gain ", they will produce the simple-monopoly output, i.e., more than the output produced without collusion. The combined profit might be divided by agreeing on an ore price at the intersection of the " manufacturer's demand curve and the supplier's offer curve "; but this would not be " a position of stable equilibrium ", because of the superior possibilities, for either party, of independent price dictation (8, p. 656).

Zeuthen (1930) was most explicit in contending that, in any monopolistic market structure, several varieties of practices, procedures and attitudes are possible and will determine the outcome. There might be a determinate price and output,[1] an indeterminate jockeying for position, economic warfare or cooperation and agreement. " The pure case in which two absolute monopolists are opposed, will in all probability seldom exist for very long without changing into a case of fighting or of cooperation " (56, p. 71). Combination will be most likely when, as in bilateral monopoly, the two parties have to deal with each other directly. " The direct negotiations between the parties in this case both as to price and sales " make an agreement the only alternative to complete withdrawal from the market (56, p. 91). " The express or tacit vertical agreement or combination will, in contradistinction to the horizontal one, result in larger sales and lower prices, but in both cases the total profit will be increased " (56, pp. 69–70).

Schneider (1932) followed Wicksell, Schumpeter, and Zeuthen in arguing that a determinate and stable equilibrium can be attained in bilateral monopoly, provided the parties are peaceful profit maximizers rather than contenders for dominance resorting to bluff and economic warfare (43, p. 72). Schneider differed from earlier writers when he rejected the supposed equivalence of bilateral monopoly with complementary monopoly on the one hand (43, p. 50), and with isolated exchange on the other (43, p. 80). He was particularly anxious to show the difference between the case of barter, where no third commodity is involved, and the case of bilateral monopolists, each of whom wants to maximize a third good, namely, money (43, pp. 78–82, 101–121). With all these deviations from widely accepted doctrine, Schneider still agreed that " the price of the ultimate product which is set when the monopolistic suppliers of productive factors cooperate will be lower than the price when the monopolists act independently . . . and hence output of final products and input of factors will be greater, and total monopoly profits higher, under cooperation " (43, p. 54).

---

[1] " Ma conclusion est donc en opposition avec la doctrine classique qui veut que le prix en cas de monopole bilatéral soit indéterminé " (57, p. 1,670).

Stackelberg (1934) rejected the conclusions reached by Wicksell, Schumpeter, Zeuthen, and Schneider, concerning the possibility of a determinate equilibrium position under bilateral monopoly (50, pp. 92–3). A fateful " *Gleichgewichtslosigkeit* "—absence of equilibrium—was the main theme of his book. The same indeterminateness also characterizes " bilateral oligopoly ", which Stackelberg was probably the first to recognize explicitly under this name. Results were supposed to be more nearly determinate, though still unstable, in situations where a monopolist was selling to oligopsonists or where oligopolists were selling to a monopsonist (50, pp. 27–29). Stackelberg mentioned the possibility, under bilateral monopoly, of options stipulating a " fixed quantity or nothing ", which might result in normal-monopoly outputs being produced rather than the more restricted outputs characteristic of independent bilateral-monopoly pricing (50, pp. 26–27). This suggestion was not taken up for several years; most writers continued to hold the Edgeworth-Bowley position.[1]

A significant advance was Henderson's (1940) demonstration that contracts between two monopolists agreeing upon the volume at the simple-monopoly quantity were much more likely than mere price-quoting methods of bargaining (23, pp. 241–43). To be sure, under the price-quoting method, one must expect " that, except by accident, the output will be less than that which could maximize joint profits so that this form of negotiation will normally be less advantageous to the parties as well as to the final consumer than the alternative " (23, p. 242). He showed, however, that

> . . . an agreement providing for definite deliveries at a given lump-sum price . . . seems to be more likely . . . [since] . . . the parties taken together cannot lose and will probably gain by adopting this form of negotiation.
>
> Now the amount . . . is that which would be produced by a single combined monopolist, so that, in the case of a fixed contract, the joint profit will be maximized and the only uncertainty is with reference to the division of that profit between the producer and the monopsonist (23, pp. 241–42).

Henderson agreed with most other writers that the division of the profits is indeterminate unless one can take account of bargaining power and bargaining skill. The division of profits or, what is the same thing, the price at which the intermediate product is transferred from the monopolist to the monopsonist does not influence the output produced or the price of the composite product to buyers at the next stage or to the ultimate consumer.

---

[1] Hicks (1935) (25, p. 16), Allen (1938) (2, p. 381, problems 28 and 29), and Tintner (1939) (52, p. 263) should be mentioned in this connection. A presentation of their views has been sacrificed to economy of space. A more complete review would also list the earlier re-statements of the problem by Leduc (1927) (27, p. 256), Hicks (1930), (24, p. 218), and Divisia (1934) (11, p. 198).

Nicholls (1941) and Leontief (1946) examined the possibilities of price-and-quantity agreements and found them probable in markets for industrial products, but improbable in agricultural (37, p. 170) and labour markets (29, p. 79).

The new bilateral-monopoly solution for industrial products—where output is determinate and equal to the output under simple monopoly—was accepted and clarified by Fellner (1947). He held that " an all-or-none clause is implicit in the passing of a product from one firm to another in all cases except those in which a price is ' quoted ' regardless of the quantity that will be taken at that price " (17, p. 525). Since the contract specifying a definite quantity yields the greatest profit it is the most likely outcome of bilateral monopoly in industrial product markets.[1]

In his survey of the literature, Haley (1948) reviewed the propositions advanced by Bowley, Tintner, Leontief and Fellner, and summarized the new solution (20, pp. 23–4). Schneider's (1949) textbook contained a detailed exposition of the determinateness of the solution—depending on the conduct of each party (44, p. 269). He spoke of price setters and price takers, quantity fixers and quantity adjusters, option proposers and option takers, where an option implies that both quantity and price are fixed.[2] Schneider concluded that the quantity transacted will be the same no matter which party proposes the option and which party takes it, with only the price (of the intermediate, not the ultimate product) differing. The quantity, moreover, will be the same as that which would secure the maximum profit to the two firms united in a merger (44, pp. 275–6).

III

BILATERAL MONOPOLY: THE PRESENT STATE OF THE THEORY

In summarizing the development of the theory of bilateral monopoly, three schools of thought can be distinguished. In the first it was concluded that both price and quantity would be determinate, but that the quantity would be lower than that fixed by an integrated monopolist, and the price to the consumer accordingly higher. In the second, it was held that both quantity and price were indeterminate within limits but, as before, the quantity would fall short of that under an integrated monopoly. The third school recognized that negotiations between separate monopolists would, in the case of intermediate products, necessarily be carried on in terms of both quantity and price, and that the quantity agreed upon between the parties would be the same as that produced by an integrated monopolist. The price to the consumer would consequently be determinate and no higher than it would be under integrated monopoly. Only the price for the intermediate product would be subject to bargaining. In other words, only the division of profits between the separate monopolists is in doubt, and fragmentation or integration makes no difference to the volume of output or to the price charged to consumers.

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[1] Fellner went much further when he asserted that bilateral monopoly will not induce any output restrictions whatsoever, but will produce the " ideal competitive output ", provided the two parties themselves are bordered and surrounded by perfectly competitive firms. He stated that not " bilateral monopoly itself " but rather monopolistic and monopsonistic positions in the preceding and subsequent stages will be responsible for output restrictions. Fellner's contention is correct only on the basis of such a wide definition of bilateral monopoly that it includes sellers and buyers who have no influence on the prices they receive or pay, respectively.

[2] These concepts were first proposed by Frisch (1933) in an essay which touched only lightly on bilateral monopoly (18, p. 243; 19, p. 25). His suggestions concerning the distribution of " action parameters " and the problem of negotiations roved eminently fruitful (18, pp. 248, 253; 19, pp. 28, 32).

Why should it have taken economists so long to arrive at the third of these answers, and why have they been unable to agree on the comparative relevance of the various models ? Why, after one hundred and twenty years' discussion, should the theory of bilateral monopoly still be " on the agenda ", as Zeuthen put it ? (58, p. 332). Why have economists most of the time assumed—following the second school of thought—that in a situation of bilateral monopoly one party would name a price only and wait for the other party to name the quantity ?

We may guess that the writers who made this assumption carried over a technique that had proved useful in the analysis of unilateral monopoly, without due examination, to the case of bilateral monopoly. A monopolist selling to many buyers quotes a price and then lets buyers take the quantities they want. A monopsonist, buying from many sellers, names a price and then lets sellers deliver the quantities they choose. This technique is not appropriate when there is only one buyer and one seller, for the two must, of necessity, negotiate and contract for a definite quantity of the intermediate product at a price to which they can agree. Economists are sometimes victims of the habit of using tools that are ready in their kit, especially if these tools are " elegant ". In this instance, the algebraic and geometric devices, involving offer curves and contract curves (in addition to indifference curves, outlay curves, and reaction curves), had done a neat job in the analysis of simple monopoly and duopoly. It was too tempting to apply them to the analysis of bilateral monopoly, too easy to overlook the fact that this application may imply assumptions concerning the behaviour of bilateral monopolists which are unsuitable in many situations.

The assumptions are not unsuitable in all cases that have been called " bilateral monopoly ". There are situations in which prices are quoted by one party, or negotiated by the two parties, while the quantity is left open. Such arrangements are common in oligopoly situations, especially when one or both parties have incomplete control over quantity because they are merely agencies for a group of buyers or sellers rather than single firms.

The case of bilateral monopoly where one party is a collective monopoly or collective monopsony is immediately obvious. A trade union in its collective bargain with a monopsonist employer will normally set only the wage rate, while the quantity of labour offered and employed is left open. It was Leontief (1946) who was the first to show that the guaranteed annual wage was one way in which the price and quantity of labour could be fixed at the same time. But in this case, the quantity would be at a riskless level, well below the actually expected volume of employment (29, p. 79). It was shown by Nicholls (1941) that an agricultural cooperative, such as a dairy farmers' cooperative, in its dealings with a monopsonist (or monemporist) distributor, will rarely be able to make price-and-quantity contracts. More often the quantity supplied by the milk producers will be left to them, and the quantity sold by the distributor in the fluid milk market will be decided by the final consumer. Only the discriminatory prices charged to fluid-milk consumers are decided upon in advance, and the lower prices for other milk products (cream, cheese, ice cream, etc.) are adjusted more flexibly (37, pp. 181–196). Similarly a cartel of several producers will, as a rule, fix only the price, leaving quantity decisions to the members and the customers. This is even more true if the oligopoly is not organized but only incompletely coordinated, and it is most certainly true in the case of an uncoordinated oligopoly.[1] Even if we were to assume, for example, that automobile manufacturers were the only buyers of steel, the price leader in the steel oligopoly could not possibly establish the quantity for the industry as a whole. Thus the bilateral oligopoly of steel and automobile manufacturers is probably more adequately analyzed by the model of the price-quoting type of bilateral monopoly than by the model featuring price-and-quantity contracts. For the case of bilateral oligopoly, consequently, the conclusion that vertical integration might lead to an increase in output could still be maintained.[2]

The one point on which nearly all economists of the twentieth century have agreed concerns the indeterminateness—in pure theory—of the division of profits between the two parties in a bilateral monopoly. Even those who concluded that the quantity was determinate and was equal to that produced by an integrated monopoly—and that the price to the next stage, or to the consumer, was determinate and equal to that charged by an integrated monopoly—agreed that the price of the intermediate product passing from the first to the second party in the bilateral monopoly was indeterminate and subject to " bargaining skill ". Depending on the price of the intermediate product (or, alternatively, on the price charged for the service rendered by one of the parties) one party or the other might get the lion's share of the combined profits. There were intimations by some economists concerning a " most reasonable " division of profits, but still the division was not determinate on the level of pure theory.[1] Whatever the division of the combined profit, it would not affect the quantity produced and the price charged to consumers. The division of profits between the two firms, if they remain independent, is, of course, subject to periodic negotiation, since all sales contracts are of limited duration. On the other hand, when the two firms merge, the division of the anticipated and capitalized profits is final. If cost and demand conditions remain unchanged, there is no reason to expect the outcome to be either more or less favourable to the consumer, one way or the other.

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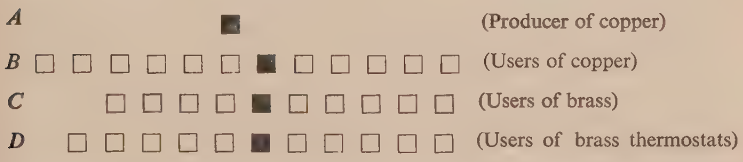
[1] Price leadership is an example of an incompletely coordinated oligopoly, while guessing-game pricing would be an example of an uncoordinated oligopoly. For detailed explanations, see Machlup (31, pp. 364–7, 491–511).

[2] This seems to be the conclusion of Stigler (51, p. 191), of Nicholls (37, p. 177), and of Hoffman (26, pp. 161–5).

IV

SUCCESSIVE MONOPOLIES: CUMULATIVE SHRINKAGES OF ELASTICITIES AND CUMULATIVE MARK-UPS

The contention that vertical integration, on account of its implications concerning market position, will increase output need not be based on any assumption that there is a bilateral monopoly between any two stages of a non-integrated industry. To clarify, we resort again to schematic illustration.



In stage *A* we may envisage a sole seller of a material, say copper, selling to a large number of buyers. One of these, *B-7*, happens to be the sole owner of zinc and is thus the only producer of brass. He has no monopsony power, since he competes with many other buyers of copper. As a monopolist in the sale of brass, he deals with a large number of buyers in stage *C*, the fabricators of brass. One of them, *C-5*, produces a patented brass thermostat for which there is no good substitute. He sells to many firms in stage *D*. One of them, *D-6*, uses this thermostat in the production of another patented product, perhaps a cooking device, which is significantly differentiated from other goods. We see here a sequence of monopolies, none of which has any monopsony power. We can conceive of an analogous pattern of successive monopsonists, none of them possessing any monopoly power. In either case the characteristics of bilateral monopoly are absent.

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[1] In terms of the model used by Bowley for the ore monopolist facing the steel monopolist, the combined profit from ore and steel making might be divided by agreeing on an ore price at the level where the marginal cost of ore would equal the steel producer's marginal net revenue product of ore. See Bowley (8, p. 656). See also Hicks (25, p. 18). The same " reasonable compromise price " was derived, perhaps independently, by Hawkins (21, p. 190).

Many economists have argued that, in such a chain of monopolists or monopsonists, each of whom makes his policy independently, final output will be smaller and the final price higher than would be the case in a vertically integrated firm. The two methods of establishing this proposition are so similar that the same authors can often be quoted on each: one method is based on the cumulative shrinkage of demand elasticities through repeated marginalization of revenue curves; the other method is based on a cumulation of price mark-ups by a succession of separate monopolists.

As Fellner puts the first argument, " the seller of the raw material . . . finds [his optimal output] by equating the marginal function corresponding to the demand curve of the buyer, that is, the function which is *marginal to the marginal value product function* of the raw material to the buyer . . . with his own marginal cost function " (17, pp. 505–6. Italics in the original).[1]

The cumulation of successive price mark-ups, which we have called the third strand in the texture of analysis, has been stressed both by writers expounding marginalist theories of the firm and by writers embracing full-cost theories of pricing. One of the marginalists, Zeuthen, discussed (56, p. 70) the case of

> . . . five entrepreneurs all in a certain fixed proportion supplying monopolised raw-materials or services for the manufacture of the same article. The price . . . will here be very much tightened up, and the quantity sold . . . strongly reduced, since each entrepreneur will fix his monopoly price at the top of that of the others. The last monopolist . . . adds the monopoly profit of the others . . . to the common costs . . . The five entrepreneurs may, however, combine, and thus . . . take the greatest total monopoly profit [on a substantially increased output].

In a later statement Zeuthen (1955) indicated that, even without complete combination, some of the same beneficial results may arise from the existence of financial liaisons, central organization, and tacit understandings (58, pp. 332–3).

It is hardly necessary to search for statements of the theory of the cumulative mark-ups by successive monopolists in terms of the full-cost principle of pricing; the idea seems almost self-evident. It is interesting, however, to find a very early critic of this hypothesis. Macgregor (1906) pointed out that it would make no difference whether a mark-up is charged on the profit margin of the supplier of an intermediate product or on the interest on his capital, which evidently will reflect the capitalized profit. Thus, " integration does not buy its material ' at cost ', if by this is meant ' prime cost '. When two capitals are joined, it still remains necessary to pay interest on both. The claim to buy at prime cost is purely a matter of bookkeeping " (30, pp. 97–8).

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[1] Similar formulations appear in Bowley (8, p. 654), Zeuthen (56, pp. 66 9), Hicks (25, pp. 17–18), Hoffman (26, p. 165), Weintraub (53, p. 308), and Hawkins (21, pp. 183–4).

Lerner (1933) and McKenzie (1951), in discussing the measurement of social losses associated with monopoly, touched upon the cumulative effects of monopoly in the separate stages of non-integrated industry, though they did not expound in detail the reason for these cumulative effects. Lerner said:

> The final degree of reduction of product will depend upon the degree of monopoly in all preceding stages. These have to be aggregated so as to give the tendency to divergence from the social optimum in the whole series of the production stages of the product. This phenomenon may be called the transitiveness of monopoly (28, pp. 171–2).

McKenzie used this idea to demonstrate the inadequacy of Lerner's " proportionality thesis "—the thesis that ideal output will exist if the degrees of monopoly in all industries are equal. If the ratios of prices to marginal costs are equal everywhere, but are greater than 1, ideal output will not be obtained. For the " social loss will be greater, the larger the number of stages of production carried on by separate firms " (33, pp. 785–803).

While it is possible to conceive of successive monopolies none of which has any monopsony power (as in the hypothetical case of the brass thermostat discussed above) it is doubtful whether such situations are very common. If *B* is a pure competitor in buying his material, he must be one of a large number of relatively small buyers—though he is a monopolist in the sale of his fabricated product. Likewise *C* and *D*, monopolists at later stages of production, are very small buyers relative to the total output of what they buy, though large relative to the markets in which they sell. Would vertical integration seem very likely in such circumstances? A small fabricator could hardly acquire a large producer of his raw material, and the large supplier may have little interest in merging with a tiny customer. On the other hand, we may perhaps assume that successive *oligopolies* may involve output restrictions without the disproportionate sizes required for successive monopolies. In such cases, as Hoffman (1940) pointed out, merger may result in a relaxation of restrictions (26, pp. 164–5).

Spengler (1950), in presenting the case for vertical integration of successive monopolies, failed to take account of the disproportionate sizes of output which would be characteristic of such situations. He showed each of the representative firms in the three stages of production as producing an output precisely adjusted to that of its supplier and/or customer, yet drew the factor cost curves of firms *B* and *C* perfectly horizontal as if they were buying as pure competitors. Moreover, the demand curves visualized by his firms A and B were not derived from the demand for C's output, as they should have been. Thus his model does not fill the specifications either of successive monopoly or of bilateral monopoly. Despite these errors he arrives at the customary conclusion:

> Vertical integration . . . has permitted our producer to evade imposts generated by horizontal integration and similar arrangements and thus reduce his selling prices below the level that would obtain in the absence of vertical integration. Vertical integration serves, therefore, to make price structures and factor allocation more ideal than they otherwise would be in an imperfectly competitive world (49, p. 351).

V

The Great Qualification

Most of the economists mentioned in this paper have presented their case for the social blessings of vertical mergers between bilateral or successive monopolies without any fundamental qualifications. Yet it must be re-emphasized that joint profit maximization between monopolists, useful though it may be in mitigating the effects of horizontal combination, is not *the* ideal form of economic arrangement when there are possibilities of achieving greater degrees of competition in the several stages. It must further be re-emphasized that joint profit maximization itself can be achieved by other means than vertical merger; loose-knit agreements and, of course, direct and free negotiations of quantities and prices may do the job.

Where direct and free negotiation between bilateral monopolists yields price-quantity contracts, it is to be preferred over tighter combinations. Periodic re-negotiations of sales contracts do, to be sure, carry with them the danger of aggravated restrictions, but they leave the way open for new competition and new arrangements. Similarly, collusion or agreement between successive or bilateral monopolists may terminate; and this may again lead to independent profit maximization with its undisciplined restrictions, but on the other hand it may lead to undisciplined competition with all its advantages for the final consumer. Vertical merger has the great advantage that it commits once-separate firms permanently to a policy of joint profit maximization, but, on the other hand, it may well solidify and expand the monopoly position itself. The vertically integrated firm which has a monopoly in one stage of an industry may extend its monopoly power into other stages or subsidiary industries where competition might otherwise survive. A vertically integrated firm may be more able to discriminate between different purchasers and thus increase its profits, or strengthen its monopoly power, or both. Finally, it may, in periods of shortage, use its control of scarce material to eliminate or prevent the growth of independent fabricators.[1]

[1] Some of these points were developed by Edwards (15).

Thus, there is good sense in a policy of discouraging or restricting vertical merger. The most important advantage of a prohibition of certain kinds of vertical merger lies, it seems, in the temptation it creates for the monopolists or monopsonists on each separate stage to integrate forward or backward by establishing new facilities. Whereas vertical integration by merger may block the increase of competition in the industry, competition may be effectively increased when firms extend their facilities to enter production at the stages of their erstwhile suppliers or customers.

Many economists, taking insufficient cognizance of these possibilities, have used the theoretical argument for vertical integration by merger as if it were a reliable guide to public policy. Not so Marshall. He combined practical sense with theoretical acumen and had the good judgment to ask whether there had not been some important possibilities locked up in the pound of *ceteris paribus*. Thus, after pointing to the "*prima facie* reasons for thinking that the public interest calls for . . . fusion" of complementary monopolies, he warns:

> But there are other considerations of perhaps greater importance on the other side. For in real life there are scarcely any monopolies as absolute and permanent as that just discussed. On the contrary there is in the modern world an ever increasing tendency towards the substitution of new things and new methods for old, which are not being developed progressively in the interests of consumers; and the direct or indirect competition thus brought to bear is likely to weaken the position of one of the complementary monopolies more than the other. For instance, if there be only one factory for spinning and one for weaving in a small isolated country, it may be for the time to the public interest that the two should be in the same hands. But the monopoly so established will be much harder to shake than would either half of it separately. For a new venturer might push his way into the spinning business and compete with the old spinning mill for the custom of the old weaving sheds (32, p. 494).

Making the same point with reference to complementary lines in the transportation industry, Marshall concluded:

> Under some conditions it is more to the public interest that they should be in one hand; under others, and those perhaps the conditions that occur the more frequently, it is in the long run to the public interest that they should remain in different hands (32, pp. 494–5).

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Towards a More General Theory of Capital[1]

By C. A. Blyth

I

This paper starts from the fact that a decision to invest in a new industrial project is a decision to invest in a flow of inputs over time in such a way as to produce a flow of outputs over time. The essential information is the quantity and value of each type of input invested, and the quantity and value of each type of output produced, at each date in the project. With this information it is possible to abstract and define the *technique of production* as the shapes of the flows of inputs and outputs over time. Further, it is possible to organise the information about alternative projects by defining a *production function* as the relationship between total output produced from a project, total inputs invested in a project, and the shapes of the input and output flows. (Output depends upon inputs and the technique of production.)

The next step is to quantify the idea of the shapes of the input and output flows. The simplification which the Austrian theorists adopted was to assert that only the *mean dates* when input was invested, and output was produced, mattered, i.e. they measured the shapes by their mean dates. Actually they took the simplification a stage further and, defining the period of production as the difference between the mean date of output and the mean date of input, they asserted that it was only variations in output arising from variations in the period of production that were important. This approach leads to the more general device of treating the shapes as statistical frequency distributions, with time as variate and frequency at any date as the ratio of input invested (or output produced) at that date to total input invested (or total output produced) over the life of the process. Frequency distributions can be defined by the familiar measures of the characteristics of such shapes: the mean date measures the central tendency; the variance measures the dispersion about the mean, etc. In general, the set of such measures, known as cumulants, is needed to define a frequency distribution.[2] With this device it is possible to present the Austrian theory in a more general form which avoids the difficulties encountered in the use of the mean dates alone (or the period of production) as a measure of technique or its derivative " capital intensity ".

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[1] An early draft of the mathematics of Section II was extensively altered after being criticised by Mr. E. Osborn. Professor R. F. Kahn helpfully criticised the paper in one of its various forms. I alone am to be held responsible for what remains.

[2] The first cumulant is the mean; the second is the variance, etc. Most statistical texts explain the measures and methods of calculation. The more familiar moments are related in a simple way to the cumulants.

The paper develops the comparative statics of a theory of capital based upon the idea that a difference in techniques means a difference in the time-shapes of the flows of inputs and outputs. Thus the production function is of the form

$$X=f(N;\ {}^{o}k_1, {}^{o}k_2, \ldots\ldots;\ {}^{I}k_1, {}^{I}k_2, \ldots\ldots\ldots) \quad \ldots\ldots\ldots\ldots(1)$$

where

X is the total physical output produced over the life of the process of production,

N is the total labour hours invested over the life of the process, the ${}^{o}k$'s and the ${}^{I}k$'s are the output and input distribution cumulants respectively, the subscripts indicating the order.

Other inputs—land, rent, goods, etc.—and multiple outputs can be easily introduced, but here I will consider only one type of input. It is initially assumed that N and the cumulants can be varied independently of each other. The significance of this assumption is considered in Section II below. The results, which stem from the use of this production function, will be compared with those of Wicksell and recent writers.

In this section my earlier results[1] are summarised in a form suitable for development, and the economic setting of the theory is described. In Section II, the relation between equilibrium output and the cumulants is analysed, and I consider the possibility of a higher real wage rate being associated with a lower output. The value of capital is examined in Section III where the difference in capital values associated with a difference in wage rates is separated into " value " and " real " parts and some conditions for a lower value of capital being associated with a higher wage rate are stated. I conclude with a Note on the connection between the essentially dynamic nature of capital theory and the static methods of analysis adopted here and by earlier writers.

The present value of a flow of expected outputs is

$$V_o = p\int_0^{\lambda} x(j)e^{-\nu j}\,dj$$

where $x(j)$ is physical output at date j,

ν is the instantaneous rate of discount,

p is the expected constant price of a unit of output,

and 0 and λ are the beginning and end dates of the process.

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[1] See C. A. Blyth, " The Theory of Capital and its Time Measures ", *Econometrica*, vol. XXIV (1956), pp. 467–79. Independently, Mr. J. D. Sargan had discovered the importance of the variance of the distributions; see his " The Period of Production ", *Econometrica*, vol. XXIII (1955), pp. 151–65. As I stated in my first paper, Dr. H. Bernardelli suggested to me the possibility of he frequency distribution approach.

V_x can be written as[1]

$$V_x = pXe^{-K_x(-v)}$$

where $K_x\,(-v)$ is a cumulant generating function of the frequency distribution of outputs.

Similarly, the present value of a flow of expected inputs is

$$V_I = wNe^{-K_I(-v)}$$

where w is the hourly wage rate, and $K_I\,(-v)$ is a cumulant generating function of the input distribution.

The instantaneous rate of return, ρ, is that rate of discount which equates the present value of inputs to the present value of outputs,

i.e. $$pXe^{-K_x(-\rho)} = wNe^{-K_I(-\rho)}$$

thus $$pX = wNe^{K_x(-\rho) - K_I(-\rho)} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

A technique is chosen so as to maximise the rate of return on the assumptions that (1) the real wage rate, w/p, is known and expected to remain constant, (2) the supply of input is perfectly elastic at this real wage rate, and (3) the choice is undertaken by a central planning authority. These assumptions imply certain types of economic environment of which one will be briefly indicated here. The ways in which quantities of standard bundles of consumption goods, X, can be produced are described by the production function. At the date when the technique is chosen, there exist stocks of consumption goods and partly finished processes. The real wage rate is determined by the planning authority on the basis of its knowledge of the stocks of consumption goods and the consumption goods which the partly finished processes can be made to produce in the future. That is to say, the investment decision, or the quantity of consumption goods which will be exchanged for labour hours, is taken as given.[2] The problem I am concerned with here is the effect of different levels of wages upon output and the value of capital, and to this end I am neglecting the problems of saving and the market determination of wages, rates of return and prices.

The conditions for maximising the rate of return are obtained by substituting (1) in (2) and differentiating by the inverse function rule. The first order conditions for the cumulants are

$$\frac{\partial X}{\partial^x k_j} \Big/ X = (-1)^{j+1} \frac{\rho^j}{j!} (j = 1, 2, 3 \ldots\ldots\ldots) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3.1)$$

$$\frac{\partial X}{\partial^I k_j} \Big/ X = (-1)^j \frac{\rho^j}{j!} (j = 1, 2, 3, \ldots\ldots\ldots) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3.2)$$

---

[1] See Blyth, *loc. cit.*

[2] I suggest in my Note below that the complete solution of the distribution problem must be made in a properly dynamic setting.

Writing $T = ({}^{x}k_1 - {}^{I}k_1) - \rho\,({}^{x}k_2 - {}^{I}k_2) + \frac{\rho^2}{2!}\,({}^{x}k_3 - {}^{I}k_3) \ldots\ldots$ and the determinant

$$\begin{vmatrix} X & \dfrac{\partial X}{\partial^{x} k_j} \\[2ex] \dfrac{\partial X}{\partial^{x} k_j} & \dfrac{\partial^2 X}{\partial^{x} k_j^2} \end{vmatrix} \quad \text{as} \quad \begin{vmatrix} X \\ j \end{vmatrix}$$

the second order conditions[1] for the output cumulants are

$$\frac{\begin{vmatrix} X \\ j \end{vmatrix}}{T} < 0 \;(j = 1, 2, 3 \ldots\ldots) \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4.1)$$

and similarly for the input cumulants

$$\frac{\begin{vmatrix} I \\ j \end{vmatrix}}{T} < 0 \;(j = 1, 2, 3 \ldots\ldots) \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4.2)$$

The first and second order conditions for N are

$$w \Big/ p = \frac{\partial X}{\partial N} e^{-[K_x(-\rho) - K_I(-\rho)]} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5.1)$$

and $\frac{\partial^2 X}{\partial N^2} < 0$ [2] $\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$ (5.2)

If the economy has at any date a set of evenly balanced processes employing identical techniques in operation (i.e. a rectangular age distribution of the lifetimes of the processes) and if the real wage is expected to remain unchanged, output at that date from all processes will equal output produced from one process over its whole lifetime, and this output will be expected to remain unchanged. The economy is in stationary equilibrium and the value of capital invested in the set of processes at any date will be

$$C = \frac{pX - wN}{\rho}$$

$$= \frac{wN\,(e^{K_x(-\rho) - K_I(-\rho)} - 1)}{\rho} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (6)$$

The following analysis is a comparison of the values of X and C associated in stationary equilibrium with different values of w/p.

---

[1] The second order conditions given in Blyth, *loc. cit.*, are wrong.

[2] These imply that the marginal product of labour equals the average product. If all cumulants except the first output cumulant are zero, we have the Jevons criterion

$$\frac{\partial X}{\partial^{x} k_1} \Big/ X = \rho.$$

II

We consider two economies each in stationary equilibrium with the same production function and employing the same labour force. The real wage in one economy (lower real wage economy) is lower than in the other (higher real wage economy). What can we say about ρ, X and C? It is obvious that the higher real wage economy has the lower rate of return. If it has a higher rate of return, say ρ_1, than the lower wage economy, that economy could earn an even higher rate of return, say ρ_2, by adopting the same technique as the higher wage economy. But in that case the higher wage economy would not be in equilibrium with a maximum rate of return: ρ_1 cannot be the equilibrium rate. The higher wage economy would adopt another technique yielding a rate of return greater than ρ_1 but less than ρ_2. Thus the higher wage economy has the lower rate of return.

The difference in the outputs of the two economies depends upon the differences in the cumulants which I interpret as the differentials $\delta{}^{x}k_j$ and $\delta{}^{I}k_j$ ($j=1, 2, 3 \ldots\ldots$). Rewrite (2) as

$$X = we^{\rho\tau} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7)$$

where $\tau = ({}^{x}k_1 - {}^{I}k_1) - \frac{\rho}{2\,!}({}^{x}k_2 - {}^{I}k_2) + \frac{\rho^2}{3\,!}({}^{x}k_3 - {}^{I}k_3) - \ldots\ldots\ldots\ldots,$

$N=1,$

and $p=1.$

The real wage is now simply w, and X is output per labour hour. I shall also use

$$T = \frac{d(\rho\tau)}{d\rho} = ({}^{x}k_1 - {}^{I}k_1) - \rho({}^{x}k_2 - {}^{I}k_2) + \frac{\rho^2}{2!}({}^{x}k_3 - {}^{I}k_3) - \ldots\ldots\ldots$$

Taking differentials of both sides of (7) we have

$$\begin{aligned}\delta X &= \delta w.e^{\rho\tau} + \delta(\rho\tau)\,w\,e^{\rho\tau}\\ &= \delta w.e^{\rho\tau} + we^{\rho\tau}\,[\delta\rho.{}^{x}k_1 + \delta{}^{x}k_1.\rho - \delta\rho.{}^{I}k_1 - \delta{}^{I}k_1.\rho\\ &\qquad - \delta\rho.\rho{}^{x}k_2 - \delta{}^{x}k_2.\frac{\rho^2}{2!} + \ldots\ldots\ldots\ldots\ldots]\end{aligned}$$

and collecting terms and dividing through by X

$$\frac{\delta X}{X} = \frac{\delta w}{w} + \delta\rho.T + \sum_j (-1)^{j+1}\frac{\rho^j}{j!}\delta{}^{x}k_j + \sum_j (-1)^j\frac{\rho^j}{j!}\delta{}^{I}k_j \quad \ldots\ldots\ldots (8)$$

Taking differentials of the production function (1) (with $N=1$) we have $\delta X = \sum_j \frac{\partial X}{\partial{}^{x}k_j}\delta{}^{x}k_j + \sum_j \frac{\partial X}{\partial{}^{I}k_j}\delta{}^{I}k_j$

and substituting the first order conditions (3.1 and 2) and dividing through by X

$$\frac{\delta X}{X} = \sum_j (-1)^{j+1}\frac{\rho^j}{j!}\delta{}^{x}k_j + \sum_j (-1)^j\frac{\rho^j}{j!}\delta{}^{I}k_j \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (9)$$

Equating the r.h.s. of (8) and (9)

$$\frac{\delta w}{w} + \delta\rho . T = 0$$

or

$$\delta\rho = -\frac{\delta w}{wT} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (10)$$

$\delta w > 0$ and we know from the argument above that $\delta\rho < 0$, thus $T > 0$.

The rate of return is a function of the cumulants, i.e.

$$\delta\rho = \sum_j \frac{\partial\rho}{\partial^x k_j} \delta^x k_j + \sum_j \frac{\partial\rho}{\partial^I k_j} \delta^I k_j \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (11)$$

From the first order conditions, using the inverse function rule, we find

$$\frac{\partial\rho}{\partial^x k^j} = \frac{\left| \begin{matrix} X \\ j \end{matrix} \right|}{X^2 \dfrac{(-1)^{j+1}}{j!} j\rho^{j-1}} (j = 1, 2, 3 \ldots\ldots.) \quad \ldots\ldots\ldots\ldots\ldots\ldots (12.1)$$

and

$$\frac{\partial\rho}{\partial^I k_j} = \frac{\left| \begin{matrix} I \\ j \end{matrix} \right|}{X^2 \dfrac{(-1)^j}{j!} j\rho^{j-1}} (j = 1, 2, 3 \ldots\ldots.) \quad \ldots\ldots\ldots\ldots\ldots\ldots (12.2)$$

Substituting (12.1 and 2) in (11) we have

$$\delta\rho = \sum_j \frac{\left| \begin{matrix} X \\ j \end{matrix} \right|}{X^2 \dfrac{(-1)^{j+1}}{(j-1)!} \rho^{j-1}} \delta^x k_j + \sum_j \frac{\left| \begin{matrix} I \\ j \end{matrix} \right|}{X^2 \dfrac{(-1)^j}{(j-1)!} \rho^{j-1}} \delta^I k_j \quad \ldots\ldots\ldots (13)$$

As $T > 0$ the determinants are all negative (from the second order conditions (4.1 and 2)). Thus a *sufficient* condition for equilibrium ($\delta\rho < 0$) is that the signs of the differentials of the cumulants be alternately positive and negative, i.e.

$$\delta^x k_1, \delta^x k_3, \ldots\ldots > 0; \quad \delta^x k_2, \delta^x k_4, \ldots\ldots < 0;$$
$$\delta^I k_1, \delta^I k_3, \ldots\ldots < 0; \quad \delta^I k_2, \delta^I k_4, \ldots\ldots > 0 \quad \ldots\ldots\ldots\ldots (14)$$

Briefly, this means that in the higher wage economy the equilibrium conditions are satisfied when the mean output date is greater, the mean input date is smaller, the input variance is greater, the output variance is smaller, etc., than the same measures of technique in the lower wage economy.[1] If the period of production is defined as $\tau_1 = {}^x k_1 - {}^I k_1$, then $\tau_1 > 0$, i.e. the period of production in the higher real wage economy is greater.

---

[1] I have adopted a convention of treating the cumulants as numbers; for the mean dates " earlier " and " later " could be used instead of " greater " and " smaller ".

Are these conditions necessary? Clearly, they are not. If the cumulants cannot be varied independently of each other, i.e. if in increasing, say, the mean output date it is necessary for technical reasons also to increase the output variance, then the conditions cannot be fulfilled and are not necessary. This situation arises if the choice of technique is restricted in the sense that certain input and output distributions cannot be chosen. Independent variation of the cumulants implies an unrestricted choice of the time shapes of the flows of inputs and outputs. If the choice is restricted to certain types of time shapes it may not be possible to consider the cumulants as independently variable. A simple example of this situation occurs when it is necessary to take a longer time over the construction of a machine if it is to be more durable (assuming the distributions are all rectangular).

On the other hand, it follows from the first and second order conditions (3.1 and 2 and 4.1 and 2) that if the cumulants can be varied independently, i.e. if there is an unrestricted choice of time shapes, the sufficiency conditions are also necessary conditions of equilibrium.

We can now investigate the sign of δX, using (9) above. There are two cases. First, if the cumulants can be varied independently, so that conditions (14) are necessary, the alternation of the signs of the $\delta^x k$'s and the $\delta^I k$'s in the r.h.s. of (9) makes $\delta X > 0$. The higher wage economy has a greater output. Second, if the cumulants cannot be varied independently, so that conditions (14) are not necessary, there need be no alternation of the signs of the $\delta^x k$'s and the $\delta^I k$'s and hence δX may be positive, negative or zero. Depending upon the range of available techniques, the higher wage economy may have an output higher than, lower than, or the same as the lower wage economy.

The case of a higher real wage being associated with a lower output has been mentioned in the literature.[1] Here I shall illustrate the result in the terms of my analysis with a numerical example. The procedure is to select the input and output distributions of two techniques, whose cumulants—I consider only the first two cumulants of each distribution—satisfy (9), making $\delta X < 0$, and (13), making $\delta \rho < 0$.[2] One technique involves the production of meat by the fattening of cattle; the other involves the production of wool by the grazing and shearing of sheep. There is only one type of input, labour hours. I suppose that at the beginning of each process labour is expended in capturing calves or lambs which are found in a wild state; and that economies which produce meat or wool trade these products for bundles of consumption goods at fixed prices—one bundle of consumption goods equals one pound of meat or one pound of wool. Thus output in each economy can be measured in units of " lbs ". Histograms defining the techniques are shown in the diagram.

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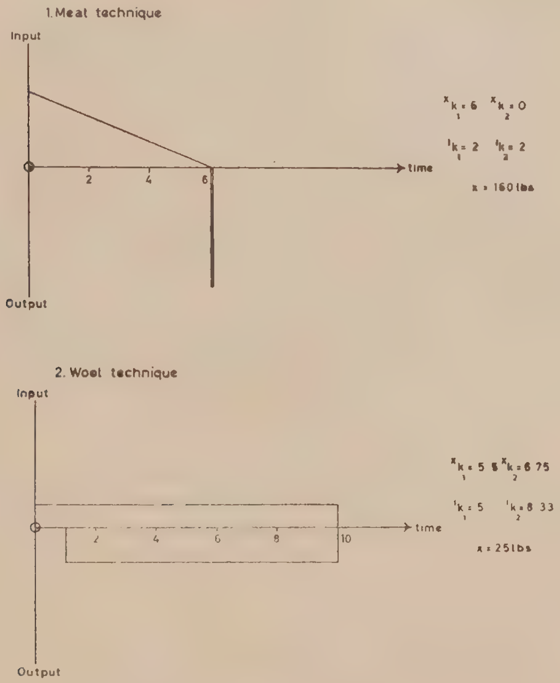
[1] Joan Robinson, *The Accumulation of Capital*, 1956, pp. 109–110, says: " As a general rule the degree of mechanisation of the technique brought over the frontier by a higher wage rate is higher [i.e. *produces a greater output*] than that corresponding to a lower wage rate, but it is possible that within certain ranges there may be a perverse relationship ". But she goes on to say: " We may therefore take it as a general rule that a higher degree of mechanisation is associated with a higher, not a lower, level of wages in terms of product ". Earlier writers discussed the proposition usually attributed to Ricardo that " a general wage rise relative to commodity prices will induce entrepreneurs to substitute machinery for labour ". (F. A. and V. A. Lutz, *The Theory of Investment of the Firm*, 1951, p. 137.)

[2] The condition regarding (13) is hardly restrictive, for we can make any assumption about the magnitude of the determinants. All that is required is that one term involving one of the δk's should differ in sign from one other term.



Two Agricultural Techniques (see text)

The meat production technique produces the greater output. Its histograms are drawn on the assumptions that in the early stages of the process much labour is invested in preparing pasture, fencing, constructing buildings, etc. and that cattle are fattened for six time periods after which they are slaughtered for beef. The wool production histograms assume that the sheep are grazed for ten time periods with a roughly constant investment of labour in shepherding and shearing. Shearing is continuous so output is continuous. Neither technique involves breeding. The feature stressed in these abstractions is that the more productive technique employs most of its labour in the early stages of the process, while the lifetime or durability of the capital good (in this case a fattened calf) is less than that in the less productive technique. In loose terms we can say that the meat technique locks up all the capital until the process ends, while the wool technique does not lock up so much capital because output comes off as a flow.[1]

We now consider two economies, A and B. A's wage is 17 units of output; B's is 18. In both economies N = 1. If A uses the meat technique the rate of return is 65 per cent. p.a.; if it uses the wool technique the rate is 63 per cent. p.a. Thus the low wage economy employs the meat technique. If economy B employs the meat technique its rate of return is 9 per cent. p.a.; if it employs the wool technique it yields 49 per cent. p.a. Thus economy B, the high wage economy, employs the less productive, wool technique.

The explanation of the phenomenon illustrated by the example is simple enough. We say (loosely) that more capital is locked up for, on the average, a longer time in the meat technique. The capital consists of labour input and accrued interest, less output and interest on the amortisation fund. Unless real wages are low enough, the locking up will reduce the rate of return below that obtainable on the wool technique which locks up less capital. We may expect the phenomenon to be a possibility whenever a greater construction time is linked with a reduced lifetime. In such cases the locking up of capital can offset greater output.

III

In this section I shall investigate the sign of δC to determine whether the higher wage economy has a greater or smaller value of capital (in stationary equilibrium).[2] As before with p and N equal to unity, rewrite (6) as

$$C = \frac{1}{\rho}(X - w)$$

and take the differentials

$$\delta C = \frac{1}{\rho^2}\left\{\rho\delta X - \rho\delta w - \delta\rho\,(X - w)\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(15)}$$

Using (10) and rearranging the terms we find

$$\delta C = \frac{1}{\rho^2}\left\{\rho\delta X - \frac{\delta w}{Tw}\left[w\,(1 + \rho T) - X\right]\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{(16)}$$

---

[1] In manufacturing industry the situation might be represented by the case where a machine can produce a greater output with a shorter lifetime than other machines, but requires a greater proportion of the labour force to be invested in its construction.

[2] It is obvious from what has been said above—especially in Section I—that the value of capital is a derived measure which need play no part in the determination of technique. The reason for the following analysis is that in fact it has played a considerable part in some writers' theories, and that it figures largely—in the capital-output ratio—in modern statisticians' attempts to place a measuring rod on the economy.

I shall discuss these results in three ways. First, I consider the meaning of (15). Second, I define the marginal product of capital so as to make it equal to the rate of return. Third, I go on to consider the effect of different wage rates on the value of capital.

First, rearrange (15) as

$$\delta C=\frac{\delta X}{\rho}-\frac{\delta w}{\rho}-\frac{\delta \rho}{\rho}\left(\frac{X-w}{\rho}\right) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(17)$$

The difference in the value of capital in the two economies has been separated into three parts. The first, $\delta X/\rho$, depends upon the difference in output which we already know depends upon differences in technique. It thus depends upon differences in the stocks of physical capital goods, and I call it the " real " part of the difference in the value of capital. If the higher wage economy has a higher output this real part is positive, and *vice versa*. The second part, $\delta w/\rho$, depends upon the difference in real wages—I call this the wage part. *Ceteris paribus* the effect of the wage part is to make the difference in the value of capital negative: for given output and rate of return, a greater wage bill means a lower interest bill and hence a lower value of capital.[1]

The third part depends upon the difference in the rates of return. It is the difference between the interest bill in the lower wage economy, capitalised at the rate of return of the higher wage economy, and the same interest bill capitalised at the rate of return of the lower wage economy. It is the difference caused by a recalculation of the value of capital using a different rate of return.[2] If we write ρ_H and ρ_L as the rates of return in the higher and lower wage economies, the difference between the two capitalised interest bills is

$$\frac{X-w}{\rho_H}-\frac{X-w}{\rho_L}=\frac{\rho_L-\rho_H}{\rho_H\,\rho_L}(X-w)$$

and in the limit when $\rho_L-\rho_H$ approaches zero this becomes $-\frac{\delta\rho}{\rho^2}(X-w)$ which is my third part. I call this the interest part and conclude that it makes the difference in the value of capital positive. The effects of the three parts are thus offsetting. In discussing the combined effect it is useful to combine the wage and interest parts as a value part as in (16).

Let us now value the difference in the value of capital in the two economies at the wage and rate of return associated with the lower wage economy. In (16) we set δw equal to zero. The second expression in the curled brackets vanishes, hence

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[1] Joan Robinson, *op. cit.*, p. 391, has called the effect of the wage part the " Wicksell effect ".

[2] T. W. Swan, " Economic Growth and Capital Accumulation ", *Economic Record*, vol. XXXII (1956), p. 355, says that " the Wicksell effect is nothing but an inventory revaluation ". (He includes both the wage part and the interest part in his Wicksell effect.)

$$\delta\bar{C} = \delta X \Big/ \rho$$

where $\delta\bar{C}$ is the difference in the value of capital valued in the way stated. Thus we obtain

$$\delta X \Big/ \delta\bar{C} = \rho \quad \dots\dots\dots\dots\dots\dots\dots\dots \quad (18)$$

I call $\delta X \Big/ \delta\bar{C}$ " the marginal product of capital " and the result is that the marginal product of capital equals the rate of return. This method of calculating a difference in capital is similar to the calculation of a chained, base weighted index.[1] Champernowne suggested a chain index of capital and arrived at a similar equality of marginal product of capital and price of capital.[2] Swan has suggested the same type of valuation in his discussion of Wicksell's treatment of the Akerman problem.[3]

Wicksell in his analysis referred to the theorem attributed to von Thünen that the rate of return is determined by the yield of the last increment of capital.[4] He concluded that the theorem is wrong. According to his definition of the marginal product of capital—$\delta X \Big/ \delta C$ in my notation—this marginal product is less than the rate of return in the wine model case, and can be greater or less in the case of machinery with no construction time and constant rate of output (the Akerman case).[5]

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[1] See for example B. D. Mudgett, *Index Numbers*, 1951, pp. 70–2.

[2] D. G. Champernowne, " The Production Function and the Theory of Capital: A Comment ", *Review of Economic Studies*, vol. XXI (1953–54), p. 115 and equation (4.4) p. 118. He has also recently shown the connection between the approach of Joan Robinson, *op. cit.*, and the chain index method: " Capital Accumulation and the Maintenance of Full Employment ", *Economic Journal*, vol. LXVIII (1958), pp. 242–44.

[3] Swan, *loc. cit.*

[4] K. Wicksell, *Lectures on Political Economy*, English translation, 1934, vol. I, p. 147 and p. 180.

[5] Wicksell, *op. cit.*, p. 180 and pp. 291–292. On Akerman's suggestion Wicksell redefined the difference in capital by subtracting that part of the difference attributable to the difference in wages, but as he assumed simple interest and neglected the difference in interest rates he could not arrive at (18) above. O. Lange (" The Place of Interest in the Theory of Production ", *Review of Economic Studies*, vol. III [1935–36]), found the marginal product of capital greater than the rate of interest. In terms of what he called " real capital "—which on his " axe " assumptions is similar to my $\delta\bar{C}$ above—he shows the equality of the marginal product of real capital and the rate of interest, and says that Wicksell apparently did not realise that von Thünen probably meant real capital (*ibid.*, p. 185, n. 2). L. A. Metzler, " The Rate of Interest and the Marginal Profit of Capital ", *Journal of Political Economy*, vol. LVIII (1950), makes the distinction which Wicksell made in his earlier work (K. Wicksell, *Value, Capital and Rent*, English translation, 1954) between the marginal private and social products of capital. (Individual firms invest at a given wage rate but all firms investing together raise wages.)

I now consider the value effect. Obviously, without assigning numbers to the variables a complete quantitative analysis cannot be made. However, it is possible to examine the value part to see whether it is positive or negative, i.e. to see whether the difference in rates of return offsets the difference in wage rates. If it does offset it, and if $\delta X>0$, then the higher real wage economy has the higher value of capital. If it does not offset it the value part may offset the real part and the value of capital may be lower.

The value part will be positive when the expression in square brackets in (16) is negative, i.e. when

$$w\ (1+\rho T)-X<0$$

or, using (2),

$$e^{\rho\tau}>1+\rho\mathrm{T} \qquad \text{.......(19)}$$

In the case of Wicksell's wine model both T and τ are equal to the time during which the wine matures. In this case the condition holds;[1] the value effect is positive and reinforces the real effect. In general it is impossible to say whether τ is greater or less than T; I shall consider an example of a flow—input—flow—output process of production (the general case of the construction and operation of durable goods). Only the first two cumulants of the distributions are used and in the expansion of exponentials I neglect terms with powers of ρ greater than 2 (" assuming usual rates of interest ").

Suppose the construction time is A units of time, the life of the durable goods is B-A, and the two distributions are rectangular.[2] The cumulants are

$$^{x}k_1=\frac{A+B}{2}\ ;\quad ^{x}k_2=\frac{(B-A)^{2}}{12}\ ;\quad ^{I}k_1=\frac{B}{2}\ ;\quad ^{I}k_2=\frac{B^2}{12}$$

hence, for usual rates of return,

$$e^{\rho\tau}=1+\rho\frac{A}{2}+\frac{\rho^2A^2}{12}+\frac{\rho^2AB}{12} \qquad \text{.......(20)}$$

and

$$1+\rho T=1+\rho\frac{A}{2}-\frac{\rho^2A^2}{12}+\frac{\rho^2AB}{12} \qquad \text{.......(21)}$$

Subtracting, we find

$$e^{\rho\tau}-(1+\rho T)=\frac{\rho^2}{6}A\left(A-\frac{B}{2}\right) \qquad \text{.......(22)}$$

The value effect is positive when

$$A>\frac{B}{2}$$

and negative when

$$\frac{B}{2}>A.$$

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[1] By the formula for the expansion of the exponential in series.

[2] The histograms would be similar to, and in the same position relative to the axes as, those in the diagram, p. 127 above, for the wool technique.

A is the construction time; $B/2$ is one half of the timespan of the process (one half of construction time plus lifetime). Half the time span of the process is the mean date of input in the distribution chosen for the example. A similar result appears when other input distributions are chosen. For example, when the input distribution is triangular and skew to the right, indicating that relatively more input is used in construction than operation, then (the output distribution as before being rectangular) the value effect is positive when

$$0.82A > B/3$$

and negative when

$$B/3 > 0.82A$$

($B/3$ is the mean date of input). The conclusion is that in the flow input—flow output process of production, representing the technique of an entire economy, embracing the extraction of raw materials, construction of durable goods and the operation of those goods, it appears that the greater the mean date of input relative to the time that elapses before output starts to be produced, the more likely it is that the value effect will be negative and hence the more likely it is that the higher real wage economy will have a lower value of capital.[1]

NOTE: THE ASSUMPTIONS OF STATIC CAPITAL THEORY

If the usual definition of dynamics—the study of dated variables—is adopted, then capital theory must always be dynamic. This is obvious from a consideration of the production function (1) used in this paper: it is a relationship between lagged and leading variables. The subject can, however, be cast in a static framework, provided the initial, given conditions of the analysis are properly specified. This involves a certain loss of generality. If this is appreciated, much previous and recent confusion about the " problem of capital in marginal productivity theories" can be avoided.[1] This note uses an example to make this clear.

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[1] This phenomenon of $\delta C < 0$ can be interpreted further by comparing two techniques. Call them X, optimum in the lower wage economy, and Y, optimum in the higher wage economy. The value of capital in stationary equilibrium is the capitalised value of the interest bill. The interest bill yielded by a process consists of (*a*) the interest bill on the inputs invested during the construction time; (*b*) the interest bill on the inputs invested in operating the durable good during its lifetime; from (*a*) and (*b*) is subtracted (*c*) the depreciation fund consisting of output and the interest earned on it. If technique Y reduces (*a*) and (*b*) while increasing (*c*) in comparison with technique X, one factor which tends to lower the value of capital has been isolated. A greater mean date of input, and a smaller construction time, do have this effect. Neglecting differences in wage rates and rates of return, it follows that if a greater proportion of input is invested at later dates, interest bills (*a*) and (*b*) will be smaller. This could occur if the construction time was smaller, if more input was required to operate the durable goods, or if the lifetime was greater. Also, if the construction time is shorter, output starts to appear earlier and hence the depreciation fund gets off to an earlier start. This factor (and also a greater output) increases the depreciation fund. There is no reason to suppose the phenomenon is unimportant in real life. Investigations of capital values and the ratio of value of capital to output do show evidence of secular declines in the capital-output ratio of several countries in the twentieth century. The observed decline may be not unconnected with the situation analysed above.

Suppose an economy has the choice of two techniques, *A* and *B*. The processes using the techniques are described in Table 1.

TABLE I

Year	Technique A		Technique B	
	Investment	Output	Investment	Output
1	4	0	1	0
2	1	0	1	0
3	1	2·4	1	1
Totals	6	2·4	3	1

Investment is measured in labour years; output in bundles of consumption goods of standard composition. The rates of return yielded by processes using the techniques for different real wage rates per year are shown in Table 2.

TABLE 2

Wage Rate per year in bundles of consumption goods	Rates of return p.a.	
	B	A
0·2	56%	50%
0·3	10·5%	22%

The rates are calculated on the assumption that the wage is expected to remain unchanged over the life of a process. At the lower wage, it is profitable to invest in processes using the *B* technique; at the higher wage the *A* technique will be employed.

Suppose at date *t* the economy is in stationary equilibrium, with a wage of 0.2. The labour force is fixed—300 men work for a conventional year. Technique *B* is employed, and 100 men are employed in new processes, 100 in one-year-old processes and 100 in two-year-old processes. Output is 100; the wage bill invested at *t* is 60; the profit bill is 40 (all in bundles). The profits can be either hoarded or consumed by capitalists. Suppose they are consumed. The situation is represented in the line for date *t* in Table 3.

The classes in the economy are capitalists and workers. Workers offer themselves for hire at the established wage (i.e. the elasticity of supply of labour is zero). During a year they consume all their wages. Capitalists possess hoards of consumption goods and stocks of partly finished consumption goods represented by partly finished processes. These two types of goods comprise the stock of capital in the economy. At the end of each year the capitalists decide how much they will invest, i.e. what quantity of consumption goods they will exchange for labour. From the viewpoint of date *t*, they have in the past decided upon 60 bundles of consumption goods.

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[1] For a recent statement of the problem, see N. Kaldor, "Alternative Theories of Distribution", *Review of Economic Studies*, vol. XXIII (1955–56).

TABLE 3

Date	Labour invested in processes using B technique			A technique			Output	Wage Bill	Wage Rate	Profits	Capitalists' Consumption	Change in Capitalists' Hoards
	New	1 yr.	2 yr.	New	1 yr.	2 yr.						
t	100	100	100				100	60	0·2	40	40	0
t+1		100	100	100			100	90	0·3	10	10	0
t+2			100	175	25		100	90	0·3	10	10	0
t+3				232·5	42·5	25	60	90	0·3	-30	10	-40
t+4				199·375	58·125	42·5	105	90	0·3	15	10	5
t+5				192·031	49·844	58·125	139·5	90	0·3	49·5	10	39·5
t+6				202·148	48·008	49·844	119·6	90	0·3	29·6	10	19·6
t+7				201·455	50·537	48·008	115·2	90	0·3	25·2	10	15·2
t+8				199·099	50·364	50·537	121·3	90	0·3	31·3	10	21·3
∞				200	50	50	120	90	0·3	30	10	20

At the end of the current year they revise their plans. They decide to increase their annual investment to 90 bundles. Each capitalist, ignorant of what the others are doing, increases his investment in order to hire more labour. In the competitive labour market the wage rate is driven up to 0.3 bundles. At this wage investment in new processes using the *B* technique is no longer profitable. New processes planned for date *t*+1 will employ the *A* technique. It will, however, be profitable to complete the partly finished processes using the *B* technique. (I assume it is not possible to alter techniques once they have been commenced, i.e., they are non-versatile.) A one-year-old process using labour at the new price yields 26 per cent.; a two-year-old process finished next year at the new wage yields 45 per cent. —compared with the rate on the *A* technique at the new wage of

25 per cent.[1] Hence at date $t+1$ 100 labour years are allocated to one-year-old processes, 100 to two-year-old processes, and 100 to new processes employing the *A* technique. Because of the lagged nature of production output is the same as before, but profits fall to 10 bundles. Suppose capitalists settle on 10 bundles as their consumption.

If the investment of 90 bundles is maintained, the economy will gradually change over to the A technique (see Table 3). At $t+3$, when processes using the *B* technique have finished, output will fall. Subsequently it fluctuates—and in the example we are considering it eventually converges upon the stationary equilibrium value of 120 bundles. The attainment of the new equilibrium depends upon the capitalists maintaining an investment of 90 bundles despite the fluctuations in output and their profits. (In the table I assume they consume 10 bundles and allow their hoards to fluctuate.)

In an economy like the one I have described, the answer to the question " What determines distribution ? " depends upon knowledge of three things: (*a*) the technical choices, (*b*) the supply of labour (or, more generally, pure inputs), and (*c*) the amount of investment. I assumed an inelastic supply of labour (300 man years) and an amount of investment (90 bundles) inelastic with respect to every other variable in the analysis—in particular, with respect to the wage rate, the rate of return, profits and hoards. These assumptions merely simplify the analysis.[2] A comparison of two equilibria is thus a comparison of different technical choices (the case of invention or shifts in the production function—which I have not been concerned with in this paper), or of different supply schedules of labour, or of different supply schedules of investment. A direct, unqualified comparison of different wage rates is thus implicitly a comparison of different investment decisions. It is a partial analysis of the distribution problem in the Marshallian sense.

The dynamic features make it likely that investment at date *t* is influenced, among other things, by investment which will be required in the future, and by the different outputs available in the future from existing capital goods (partly finished processes) depending upon the way the goods are used. The importance of the existing capital stock and the ways it can be used is evident. It is obvious that by a stretch of the imagination the capital stock can be considered as a fund out of which wages are paid. The difficulty raised by this approach is that the wage and the use of the capital stock are determined interdependently. It is this fact that accounts for the criticism of the marginal productivity theory of distribution that the stock of capital cannot be taken as datum.[1] The units in which the stock is measured are not invariant to changes in other economic variables—in particular, to changes in the wage rate.

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[1] The owners of the old processes will experience capital losses on the basis of their previous expectations of a rate of 56 per cent. If there is a capital goods market the prices of goods in process of production will rise, because the owners of the old processes can still obtain a yield higher than that obtainable elsewhere.

[2] Other assumptions would make the problem of discovering whether the process converges on a new equilibrium—stationary or otherwise—much more difficult.

I have argued here by example that the best approach is to drop capital from capital theory. We start with the technical choices of use of given stocks of goods and the desires of their owners. Wage rate and amount of investment are mutually determined along with the other quantities. The comparative static analysis presented in this paper follows the traditional lines and, short-circuiting several of the relationships, takes the amount of investment as given.[2]

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[1] See F. A. Hayek's criticism of the fund idea, *The Pure Theory of Capital*, 1941, chap. VII. Böhm-Bawerk probably sinned in this direction; it is arguable whether Wicksell did.

[2] It is obvious that although the argument of this note has been concerned with wages, it applies to the prices of other inputs also, and in particular to the prices of durable capital goods used as inputs in a process of production. It thus avoids the so-called circularity problem (that prices depend on rates of return) which emerges in a partial analysis.

Some Effects of the $1.00 Minimum Wage in the United States

By H. M. Douty

I. INTRODUCTION

The United States has now had 20 years of experience with a national minimum wage.[1] Under the Federal Fair Labor Standards Act, the minimum rate applies uniformly to covered employees throughout the continental United States. By action of the Congress, the rate was raised from 75 cents to $1.00 an hour, effective March 1, 1956. Subsequently, plans were made for a series of studies designed to throw light on the economic impact of the higher minimum. This programme has provided the largest body of data ever assembled in the United States bearing directly on this question. The present paper seeks to describe the programme that was undertaken and to summarize some of its results.

By way of background, the statutory minimum rate currently applies to approximately 24 million employees. Since there are about 43 million employees in private non-agricultural establishments, coverage is far from universal. The test for coverage under the Federal law is whether an employee is engaged in, or producing goods for, interstate commerce. The principal employee groups excluded from coverage, either because of specific exemptions or because they fail to meet the interstate commerce test, are executive, administrative, and professional employees, outside salesmen, and most employees in retail trade, service industries, and construction.[2] Agricultural labourers and domestic servants are also excluded.

A change in the statutory minimum rate is typically made only after elaborate consideration of the question by the Congress. Interested parties, including those from the executive branch of the government, are given ample opportunity to be heard.[3] During the course of the hearings, available statistical data on wages, prices, productivity, and related matters are examined.

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[1] For an excellent summary of the major developments in Federal minimum wage legislation through 1958, see Harry S. Kantor, " Two Decades of the Fair Labor Standards Act ", *Monthly Labor Review*, October 1958, pp. 1097–1106.

[2] As of December 1958, approximately 4.2 million workers, mainly in retail trade and service industries, were covered by minimum wage laws in 24 States and the District of Columbia. In many cases, the applicable minimum rates are below the Federal level.

[3] With reference to the amendment increasing the minimum wage to $1.00 an hour, see *Hearings on Amending the Fair Labor Standards Act of 1938*, Sub-committee on Labor, Committee on Labor and Public Welfare, United States Senate (84th Congress, 1st Session); and *Hearings on Amendment to Increase the Minimum Wage*, Committee on Education and Labor, House of Representatives (84th Congress, 1st Session). The printed record of these two hearings exceeds 3,000 pages.

In preparation for the hearings that led to the increase in the minimum wage from 75 cents to $1.00, the Bureau of Labor Statistics conducted a survey to determine the distribution of factory workers by wage rates.[1] Factory workers are the largest single group presently covered by the Act; general knowledge of wages in other covered employment suggested that an increase in the minimum probably would have more effect on the wages of factory workers than on the wages of any other large group of covered employees. At the time of the survey, the hourly rates (or earnings) of factory workers averaged $1.68 an hour. Approximately 10 per cent (1.3 million) of the workers earned various amounts below $1.00. These workers were not, of course, evenly distributed throughout factory employment. They were, in fact, rather heavily concentrated in certain industries and in certain geographic areas, principally in the South. Regionally, the proportions of factory workers earning less than $1.00 were 8 per cent in the Northeast, 28.2 per cent in the South, 4.1 per cent in the Middle West, and 1.8 per cent in the Far West.

In general, the differential effect on wages of any increase in the minimum was undoubtedly given great weight in the decision that was ultimately reached. The Congress rejected the trade union proposal for a minimum rate of $1.25; it also rejected the initial proposal advanced by the executive branch of the government for a rate of 90 cents. It was the judgment of the Congress that a rate of $1.00 was the highest that could be imposed without, in the language of the Act, " substantially curtailing employment or earning power ".

II. THE SURVEY PROGRAMME

The programme of economic effects studies was planned jointly by the Wage and Hour and Public Contracts Divisions, which have responsibility for the administration of the Fair Labor Standards Act, and the Bureau of Labor Statistics. The actual survey work was conducted by the Bureau, which has issued reports on the individual surveys included within the programme. Utilizing these survey data and other relevant information, the Wage and Hour and Public Contracts Divisions have the task of overall evaluation of the effects of the higher minimum wage.[2]

The survey programme included a variety of studies.

1. Detailed surveys were made in 12 manufacturing industries, or segments of industries, that were known to have been heavily

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[1] U.S. Department of Labor, Bureau of Labor Statistics, Bulletin No. 1179, *Factory Workers' Earnings, April 1954* (Washington: Government Printing Office, 1955).

[2] To date, the Wage and Hour and Public Contracts Divisions have issued an interim Report entitled *Studies of the Economic Effects of the $1.00 Minimum Wage* (March 1957). Additional materials on the effects of the $1.00 minimum are incorporated in the Annual Report (January 1959) by the Secretary of Labor to the Congress, as required by the Fair Labor Standards Act.

affected by the $1.00 minimum.[1] Data were collected by personal visits of Bureau representatives to probability samples of establishments in each industry for single payroll periods prior to, immediately after, and, typically, one year after, the effective date of the higher minimum. In the case of certain markedly seasonal industries (canning, raw sugar, and tobacco stemming), the payroll dates related to periods of high operation in 1955, 1956, and 1957. The information collected included wage rates or straight-time earnings for all individual non-supervisory workers and separately for workers in selected occupations, employee benefit practices, and general information on the ways in which employers adjusted to the higher minimum.

2. Studies were made of wage distributions in both *covered* and *non-covered* employments as of February and April 1956 and April 1957 in seven comparatively small communities where it was known that the higher minimum rate had a significant impact on covered employment.[2] Data were collected by Bureau representatives from samples of establishments in both the covered and non-covered segments.

3. Two other relevant studies should be mentioned, although their results are not utilized explicitly in this paper. The first is a survey of the distribution of factory workers by wage rates or straight-time earnings as of April 1956 in five broad industry groups (food, textiles, apparel, furniture, and leather).[3] The second is a comprehensive survey of wage distributions in manufacturing as of May 1958, comparable with the 1954 study referred to earlier.[4]

It should be stated plainly that these various studies do not answer all of the questions that economists raise with respect to the economic consequences of a minimum wage. The principal reasons are the limits to the reporting burden that can be imposed upon employers;

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[1] The results of the surveys were issued in the numbered series of B.L.S. Reports and may be identified by the following numbers: 111 and 132 (fertilizer); 112 and 129 (seamless hosiery); 113 and 130 (southern sawmills); 115 (footwear); 115 and 124 (processed waste and work shirts); 115 and 126 (wooden containers); 116 (men's and boys' dress shirts and nightwear); 117 (cigars); 117 and 136 (canning and freezing of raw fruits and vegetables, raw sugar processing, and tobacco stemming and redrying). Where two numbers are shown, the first relates to the report showing survey results for the periods prior to and immediately after the effective date of the higher minimum; the second number relates to the report containing survey results for a period one year later. See also the summary articles by Norman J. Samuels in the *Monthly Labor Review*, as follows: "Effects of the $1 Minimum Wage in Seven Industries," March and April 1957; "Effects of the $1 Minimum Wage in Three Seasonal Industries," September 1957; "Effects of the $1 Minimum Wage in Five Industries", May 1958; and "Plant Adjustment to the $1 Minimum Wage", October 1958.

[2] B.L.S. Reports Nos. 114–3 and 127–2 (Athens, Georgia); 114–4 and 127–4 (Dalton, Georgia); 114–5 and 127–1 (Sunbury-Shamokin-Mt. Carmel, Pennsylvania); 114–6 and 127–5 (Dothan, Alabama); 114–7 and 127–3 (Hickory, North Carolina); 114–8 and 127–6 (Fort Smith, Arkansas); 114–9 and 127–7 (Meridian, Mississippi). See also the summary article by Louis E. Badenhoop, "Effects of the $1 Minimum Wage in Seven Areas", *Monthly Labor Review*, July 1958.

[3] B.L.S. Report No. 118, *Factory Workers' Earnings in Five Industry Groups, April 1956*.

[4] B.L.S. Report No. 144, *Factory Workers' Earnings, May 1958*.

the lack of availability of records to match precisely the concepts of economic analysis; and the fact that statistical studies can rarely isolate the several variables that affect through time the wage-employment relationship. Nevertheless, the group of studies in the programme under review do yield many useful and suggestive results.

III. INDUSTRY WAGE EFFECTS

The increase in the minimum wage from 75 cents to $1.00 an hour on March 1, 1956, sharply increased wage levels in low-wage industries. This is seen in Table 1, which presents a few figures from the industry surveys referred to above.[1] The increases in average hourly earnings from the first to the second payroll periods shown in the table ranged from 5.3 per cent for cigar manufacture in the Southeast and citrus fruit canning in Florida to 21.7 per cent for southern work shirt producers. The southern sawmilling industry, employing approximately 140,000 workers, experienced an increase of 17.6 per cent.

The most conspicuous immediate consequence of the higher minimum rate was severe compression of the wage structures of the heavily affected industries. This is suggested in Table 1 by the concentrations of workers in the $1.00–$1.05 wage interval in the period immediately following the introduction of the $1.00 minimum.[2] These proportions (except in work shirts) had declined moderately by the end of the first year of operation under the higher minimum, reflecting comparatively small additional increases in wage levels.

It is often said that the institution of a higher minimum wage requires that wage increases be granted all along the line to maintain interpersonal and occupational wage differentials. But this typically is not the case. In low-wage industries, both types of differentials tend in the short-run to be substantially narrowed.

In the industries under review, many establishments initially granted only those wage increases required to bring workers earning various amounts below $1.00 up to that level. However, additional adjustments were made by some firms, as reflected in the increases in the proportions of workers earning $1.05 or more between the first and second payroll periods shown in Table 1. The relationship between the initial "direct" and "indirect" impact of the higher minimum can be calculated from the detailed wage distributions in the survey reports. In the southern sawmilling industry, for example, an increase of about 15.4 per cent was required solely to bring workers earning various amounts below $1.00 up to that amount. The actual increase was 17.6 per cent. It would thus appear that in this industry the immediate

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[1] See footnote 1, p. 139.

[2] The small proportions of workers in a number of the industries earning less than $1.00 an hour after March 1956 do not, for the most part, reflect violations of the law. Establishments engaged solely in intrastate trade are not covered and there are certain specific exemptions in covered establishments; learners and handicapped workers, under proper procedures, may be employed at rates below the legal minimum.

TABLE 1

STRAIGHT-TIME AVERAGE HOURLY EARNINGS AND PROPORTIONS OF WORKERS IN VARIOUS WAGE CLASSES, SELECTED INDUSTRIES AND PAYROLL PERIODS

Industry and payroll period	Straight-time average hourly earnings	Proportions of workers earning—		
		Less than $1.00	$1.00 and under $1.05	$1.05 or more
Sawmills (South)—				
Autumn 1955	$0.91	73·5	10·6	15·9
April 1956	1.07	3·4	69·3	27·3
April 1957	1.09	2·6	65·2	32·2
Fertilizer (South)—				
April 1955	1.10	40·6	10·0	49·4
April 1956	1.20	4·9	32·5	62·6
April 1957	1.25	3·5	28·3	68·2
Men's Seamless Hosiery—				
February 1956	1.11	39·2	10·8	50·0
April 1956	1.20	1·8	34·2	64·0
April 1957	1.22	1·9	29·7	68·4
Processed Waste (South)—				
February 1956	.92	79·8	3·9	16·3
April 1956	1.09	—	66·7	33·3
April 1957	1.10	—	60·2	39·8
Wooden Containers (South)—				
February 1956	.94	75·6	7·7	16·7
April 1956	1.11	·1	65·1	34·8
April 1957	1.13	·2	58·7	41·1
Work Shirts (Southeast)				
February 1956	.92	73·8	7·5	18·7
April 1956	1.12	1·5	42·8	55·7
April 1957	1.11	·6	44·3	55·1
Dress Shirts and Nightwear—				
February 1956	1.09	46·0	8·2	45·8
April 1956	1.19	1·9	40·8	57·3
October 1956	1.22	2·9	35·2	61·9
Footwear (South)—				
February 1956	1.17	29·9	11·2	58·9
April 1956	1.24	·4	37·0	62·6
Cigars (Southeast)—				
February 1956	1.13	29·4	9·7	60·9
April 1956	1.19	3·4	28·4	68·2
October 1956	1.21	3·3	25·2	71·5
Raw Sugar Mills (Louisiana)—				
Autumn 1955	.88	80·1	4·7	15·2
Autumn 1956	1.10	—	58·3	41·7
Autumn 1957	1.11	—	57·1	42·9
Tobacco Stemming (North Carolina)—				
Autumn 1955	.89	79·7	4·3	16·0
Autumn 1956	1.07	—	76·9	23·1
Autumn 1957	1.11	—	70·7	29·3
Canning and Freezing (Florida)—				
Winter 1956	1.13	32·0	15·8	52·2
Winter 1957	1.19	—	30·0	70·0
Winter 1958	1.27	—	17·1	82·9

SOURCE: Bureau of Labor Statistics, U.S. Department of Labor.

" indirect " impact on wages was only approximately one-seventh as great as the " direct " impact. But the complete maintenance of the relative wage structure that had existed in sawmilling prior to the $1.00 minimum would have required an " indirect " impact somewhat greater than the " direct " impact.

The data for the third payroll period in Table 1 reflect wage changes that occurred during the year following the initial adjustments to the higher minimum. Except for work shirts, there is evidence of some widening of the wage distributions and hence some restoration of differentials. The gradual nature of the process can again be illustrated by reference to southern sawmilling, which the Bureau has surveyed repeatedly in recent years. Table 2 shows the relationship between the average earnings of machine off-bearers, an unskilled labouring job, and two relatively skilled occupations for various years from 1949 to 1957. It will be noticed that the level of earnings of the two skilled jobs declined between 1949 and 1950 relative to the level for off-bearers. This decline coincided with the introduction of the 75 cent minimum wage, effective January 25, 1950. The 1949 relationship had been partially restored by 1953 and completely regained by 1955. Skill differentials again declined in 1956, when the minimum was raised to $1.00, with some restoration evident in the following year.

TABLE 2

SOUTHERN SAWMILLING: WAGE RELATIONSHIPS, SELECTED OCCUPATIONS, 1949–57
(Average straight-time hourly earnings of machine off-bearers = 100)

Occupation	1949	1950	1953	1955	1956	1957
Machine off-bearers ..	100	100	100	100	100	100
Circular-head-saw operators	170	153	164	169	147	149
Band-head-saw operators	216	195	206	217	187	192

SOURCE: B.L.S. Report No. 130, Wage Structure: Southern Sawmills, April 1957.

The initial decline in job differentials is not surprising. A rise in the legal minimum wage does not alter the underlying market situation for the types of labour employed by an industry. Except for considerations of morale, there is no evident immediate need for firms to grant increases to workers in higher skill classifications, especially to those whose wages are already substantially above the minimum. The situation would appear to be different in circumstances where employees in essentially similar work, but differing considerably in personal efficiency, earn various amounts below the new minimum. If the wages of these workers of varying efficiency are raised to a uniform level (i.e., to the new minimum), the old structure of efficiency wages is destroyed. The elimination of these differentials through the compression of the lower end of the wage scale sets counteracting forces in motion. It is among the less efficient workers in this group that discharges are most likely to occur. Hiring standards will tend to be raised. Technical innovations (e.g., mechanical stacking equipment in the case of sawmills) affecting the relatively unskilled areas of work will tend to be introduced, and, assuming no increase in product demand, will provide a basis for some curtailment of employment. These and other forces will tend over a number of years to restore wage distributions to more " normal " proportions.

The initial impact of a higher minimum wage on low-wage industries also results in a decline in differentials among establishments classified by such factors as location, size, unionization, or type of operation or product. Although the former differentials will tend to be re-established over a period of years, the dynamics of industry are such that exact correspondence in a later period should not be anticipated.

IV. " OUTWARD " WAGE EFFECTS

Table 3 shows summary data on the level and distribution of hourly earnings for industries covered by the higher minimum and for industries not covered in three small labour markets immediately before and after the effective date of the $1.00 minimum and one year later. The results are illustrative of those obtained for all seven of the markets in which such studies were made.[1] The primary purpose of this group of surveys was to determine the behaviour of wages in non-covered employments in labour markets in which an increase in the minimum wage had a significant impact on the wages of covered workers. The non-covered workers were employed largely in retail trade and in various service industries.

As Table 3 indicates, comparatively large proportions of workers in both covered and non-covered industries were employed at various rates below $1.00 an hour in February 1956. When the minimum rate was raised to $1.00 on March 1, 1956, sizeable wage adjustments occurred in the covered industries as workers were moved up to the $1.00 rate. In non-covered employments, however, the shifts in the wage distributions were negligible. There was no appreciable immediate " outward " effect in these small labour markets.

Over the following year, additional wage adjustments occurred in the covered industries, reducing somewhat the concentrations of workers at the $1.00–$1.10 interval. In non-covered industries, wage adjustments between April 1956 and April 1957 tended to be relatively greater (and usually greater in absolute terms) than in covered industries. But these increases were not spectacular, and probably would have occurred, certainly in part, in the absence of the " wage push " provided to covered industries by the higher minimum wage. By

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[1] See footnote 2, p. 139.

TABLE 3

STRAIGHT-TIME AVERAGE HOURLY EARNINGS AND PROPORTIONS OF WORKERS IN VAROUS WAGE CLASSES IN COVERED AND NON-COVERED INDUSTRIES, THREE SMALL LABOUR MARKETS, FEBRUARY AND APRIL 1956, APRIL 1957

Area, industry group and payroll period	Straight-time average hourly earnings	Proportions of workers earning—		
		Less than $1.00	$1.00 and under $1.10	$1.10 or more
Athens, Georgia—				
Covered industries—				
February 1956	$1.09	47·8	18·3	33·9
April 1956	1.21	·7	60·0	39·3
April 1957	1.25	·4	47·6	52·0
Non-covered industries—				
February 1956	.93	60·9	9·1	30·0
April 1956	.94	58·0	10·0	32·0
April 1957	.97	58·1	8·8	33·1
Hickory, North Carolina—				
Covered industries—				
February 1956	1.16	28·1	21·3	50·6
April 1956	1.22	1·1	39·4	59·5
April 1957	1.27	·4	32·7	66·9
Non-covered industries—				
February 1956	.90	67·6	6·9	25·5
April 1956	.90	67·8	7·5	24·7
April 1957	.96	57·7	8·9	33·4
Meridian, Mississippi—				
Covered industries—				
February 1956	1.14	45·1	11·3	43·6
April 1956	1.26	3·8	40·9	55·3
April 1957	1.30	3·6	36·2	60·2
Non-covered industries—				
February 1956	.82	69·4	6·8	23·8
April 1956	.84	64·1	9·6	26·3
April 1957	.91	58·2	10·2	31·6

SOURCE: Bureau of Labor Statistics, U.S. Department of Labor.

April 1957, the proportions of workers earning less than $1.00 in non-covered industries remained rather strikingly large.

The comparisons between covered and non-covered employments up to this point have related to different industries in the same labour markets. The data suggest that the " outward " effect of the higher minimum in these situations was at best small. This does not seem to be the case, however, within particular industries containing segments of non-covered employment. For example, the Act exempts logging workers in sawmills that employ 12 or fewer logging employees. Although not required to do so by law, most of these mills in the South increased the wage rates of their logging workers along with their sawmill workers. With the advent of the $1.00 minimum, the proportions of loggers earning less than this amount declined from 71 per cent to 3 per cent in mills with 12 or fewer loggers. In the fertilizer industry in the Southeast, to give another example, about one-fourth of the workers are employed by firms selling entirely in intrastate markets and hence not legally subject to the Federal minimum wage. Nevertheless, the level of hourly wages in these intrastate firms increased substantially (by 13 cents an hour) when the $1.00 minimum became effective; the proportion of workers earning less than $1.00 declined from 83 per cent to 27 per cent between the peak production seasons of 1955 and 1956.

One may conclude that the impact of a higher minimum wage is likely to be significant with respect to non-covered employees in low-wage industries in which a substantial proportion of the workers are covered. The impact is not likely to be substantial in the case of industries that are not legally subject to the minimum at all, even in labour markets where covered industries are heavily affected.

V. Employment Effects

The employment effects of the higher minimum can be analyzed with much less assurance than the wage effects. For one thing, data in the surveys under review relate to specific payroll periods, and hence do not provide a continuous record of employment. It is difficult, in any case, to disentangle the wage factor from other factors affecting the level of employment in the industries concerned. Although the period was one of generally buoyant economic activity, the forces affecting product demand in particular industries were far from uniform. Among the industries studied, for example, wooden containers appears definitely to be a " declining " industry. In the canning and freezing industries, crop conditions heavily influence the level of operations during the canning season. In the case of seamless hosiery, there was evidence of substantial production for inventory in the months immediately preceding the effective date of the higher minimum.

Although not capable of precise measurement, there were employment effects in low-wage industries. A crude indication is that in all except one of the manufacturing industries studied,[1] employment declined between the period preceding and immediately after the effective date of the higher rate. Perhaps more significantly, employment one year after the effective date was lower than in the pre-$1.00 period in all except two of the industries, the declines ranging from 3.2 per cent to more than 15 per cent. Nominal employment increases were registered in two industries. When the plants in the industries studied were classified by degree of impact of the minimum wage, there was a distinct tendency for employment to decline more or to increase less in those plants where the impact was " high " than where it was " low ".[2]

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[1] The reference again is to the industries listed in footnote 1, p. 139.

[2] Degree of impact was defined in terms of the increase in establishment wage bills required to raise workers earning less than $1.00 up to that amount.

The fact should be re-emphasized that employment experience in these industries reflected factors other than the minimum wage. But the influence of the higher minimum can be seen in the answers to inquiries on the nature of plant adjustment to the increased payroll costs. These answers provide qualitative rather than statistical insight into employment effects. As one would expect, the adjustments were various and frequently more than one type was utilized by the same firm. They included the discharge of workers, failure to replace workers, greater care in the selection of workers, a rise in production standards, the more efficient arrangement of production sequences, increased investment in machinery and equipment, and product price increases.

VI. CONCLUSIONS

What has been attempted here is a very broad summary, involving great sacrifice of detail and qualification, of some of the results of a series of intensive studies in selected low-wage industries and labour markets of the impact of an increase in a uniform minimum wage. The structure of wages in the United States is such that many industries and geographic areas were affected, if at all, only in the most marginal sense. But other industries and areas were heavily affected and some of the consequences of the higher legal rate could be readily discerned. The time period involved in the surveys was essentially short-run.

It is clear that the higher rate was effective, in the sense that large numbers of workers earning less than $1.00 an hour were raised at least to that level. Such workers numbered more than one million in manufacturing alone and approximately two million of the total number covered by the Fair Labor Standards Act. At the same time, occupational and other types of wage differentials were sharply reduced in heavily affected industries. There was evidence of some restoration of differentials in the year after the effective date of the higher rate, although this process is likely to be comparatively gradual. The higher legal rate had little immediate impact on wages in non-covered industries, even in labour markets where the effect on covered employments was relatively great. The impact was substantial, however, on the wages of non-covered segments of employment in predominantly covered low-wage industries. Employers sought in a variety of ways to adjust their operations to the higher wage standard, with evidence of some employment displacement in heavily affected industries. Precise statistical measurement of the employment effects of a minimum wage is virtually impossible in view of the diverse nature of the forces affecting output and employment that impinge on particular industries. Moreover, the full adjustment may well take a considerable period of time, since the short-run demand for labour appears to be relatively inelastic.

Experience thus far under the Fair Labor Standards Act indicates that a uniform minimum wage can be used to improve wage standards in relatively low-wage industries where labour markets are such that wage adjustments tend to lag behind those in the economy generally. The price is some employment displacement, which is inevitable in the enforcement of a socially determined wage standard requiring significant wage adjustments in some sectors of employment.

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Economists' Cost Rules and Equilibrium Theory

By G. F. THIRLBY

This is an article about rules, devised and advocated by certain economists, for the control of business and other undertakings: rules, such as the one which says that marginal cost ought to be equal to price, which prescribe what are claimed to be correct relationships between an undertaking's cost and revenue. I have attacked the rules before, in a paper[1] which has received some notice.[2] The following observations, on four crucial and interconnected issues, succeed a re-scrutiny of some of the fundamental theoretical literature[3] which must have guided me in the preparation of that article and one which immediately preceded it.[4] While I am still following the same line of attack, seeking to expose the non-objectivity and non-implementability of the rules, I am this time offering a little more than a hint that the rules ought to fall away with the ground on which they were built, that is to say, with the notion of perfect competition or competitive equilibrium. This notion requires, I believe, to be replaced by a different notion of equilibrium which was, I feel, implicitly recognised in the earlier of my two articles to which I have just referred.

1. CONFLICTING NOTIONS OF RATIONALITY

In economics, the human being is supposed to pursue ends, valued and chosen by himself, with the use of means of some kind.[5] This behaviour, including the choice, is spoken of as rational behaviour. But the content of the conception of rationality varies. Sometimes it is adjusted to admit the behaviour of the ordinary, sane, human being who is limited in his knowledge[6], and can make mistakes in the processes whereby he selects and pursues his ends. Sometimes it is something less fallible than this, in that it includes “knowledge” that the behaving subject does not possess, or objective data of which the behaving subject may be unaware.[1] At the extreme, it is something which is omniscient and infallible: something which has, or is, perfect foresight.[2] It was, I believe, because of an attitude which tacitly assumed that the behaving subject was possessed of a rationality which transcended his actual state[3] that it became possible to put forward the rules as implementable controls. The appropriate assumption is the other one: that the human being is limited and fallible.

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[1] " The Ruler ", *The South African Journal of Economics*, December, 1946.

[2] Latterly in J. Wiseman, " The Theory of Public Utility Price: An Empty Box ", *Oxford Economic Papers*, February, 1957.

[3] Particularly Lionel Robbins, *The Nature and Significance of Economic Science*, Second Edition, 1935; Lionel Robbins, " Remarks upon Certain Aspects of the Theory of Costs ", *Economic Journal*, March, 1934; F. A. Hayek, " Economics and Knowledge ", *Economica*, February, 1937; F. A. Hayek, " Scientism and the Study of Society ", *Economica*, August, 1942, February, 1943 and February, 1944.

[4] " The Subjective Theory of Value and Accounting ' Cost ' ", *Economica*, February, 1946.

[5] Cf. Lionel Robbins, *The Nature and Significance of Economic Science*, p. 18.

[6] It is so adjusted in the following statement. " It may be irrational to be completely consistent as between commodities . . . just because the time and attention which such exact comparisons require are (in the opinion of the economic subject concerned) better spent in other ways. In other words, there may be an opportunity cost of ' internal arbitrage ' which, beyond a certain point, outweighs the gain ". (Robbins, *ibid.*, p. 92). This opportunity cost of " internal arbitrage ", which is introduced to limit the pursuit of maximisation, is referred to alternatively as " the marginal utility of not bothering about marginal utility ".

The act of choice, which is part of the rational behaviour, involves the rejection of a course of action to achieve a value which is called “cost”, and the selection of a course of action to achieve another value which, in the context of the rules, is often called “revenue”. These two values, “cost” and “revenue”, are the subject of the rules.

2. Confusion on the Meaning of “Cost”

In the consideration of the behaviour of isolated man in the non-market environment (Robinson Crusoe), it is admitted that the two values compared (the “cost” and the other one) are values of alternative end-products that might be achieved by the use of the same resources. The full application of the same doctrine to the entrepreneur in the exchange economy would require the cost to be regarded as the entrepreneur's own valuation of the outcome of a course of action that he rejects. And because, as a first approximation, money revenue is regarded as the entrepreneur's single aim, this outcome would be an alternative money revenue.[1] But the doctrine is not always fully applied in this way. The rejected course of action with its outcome of money revenue tends to be ignored, and to be replaced (as the " cost " element to be compared with the " revenue " element) by the entrepreneur's resources themselves (these being assumed to consist of money) or, what amounts to the same thing, by the " prices " of the factors to be bought and used in the accepted course of action.[2] At this point, " cost ", defined as money resource input, or outlay, relinquishes its connection with the end-value of the use of the entrepreneur's resources in an alternative course of action, and consequently with the entrepreneur's act of choice.

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[1] Robbins appears to switch to this view of rationality in giving an instance of inconsistency which can be shown up by economics: the inconsistency of wishing to satisfy consumers' demands fully and at the same time wishing to impede the import of foreign goods by tariffs. (Robbins, *ibid.*, p. 92.) He refers to this as irrationality. It would seem to be open to an advocate for the person having these inconsistent wishes to plead that the person had not the knowledge to show that the wishes were inconsistent, that he preferred (to the marginal utility of the extra knowledge) the marginal utility of not bothering about it, and that consequently he was rational according to Robbins' earlier view. (See previous footnote.) At least we may say that if, in this instance, the behaving subject is irrational through ignorance, rationality implies knowledge greater than the behaving subject possesses, and consequently we are able to call a man rational or irrational according to which of Robbins' conceptions we use.

[2] “The fact is, of course, that the assumption of perfect rationality in the sense of complete consistency is simply one of a number of assumptions of a psychological nature which are introduced into economic analysis at various stages of approximation to reality. The perfect foresight, which it is sometimes convenient to postulate, is an assumption of a similar nature”; and “Rationality in choice is nothing more and nothing less than choice with complete awareness of the alternatives rejected”. (Robbins, *ibid.*, pp. 93–4, and p. 152 respectively.)

[3] And, possibly, confused the rationality of the behaving subject with the rationality of an observing economist who was assumed to be omniscient. See Hayek's discussion of the confusion about the data, or facts, of the demand schedules, where he raises “the question whether the facts referred to are supposed to be given to the observing economist, or to the persons whose actions he wants to explain, and if the latter, whether it is assumed that the same facts are known to all the different persons in the system, or whether the ‘data’ for the different persons may be different” (F. A. Hayek, “Economics and Knowledge”, p. 39).

But this is not all. The subtle change in the meaning of cost, from the valuation of his *own* displaced end-product to the money input required for the selected course of action, is a change leading to still another conception, which carries with it the suspicion that it is to be regarded as a social cost. It resembles the first meaning of cost, in that it is supposed to be an alternative value displaced, but differs from it in that it is not the entrepreneur's *own* valuation of his *own* displaced end-product, but other people's (consumers') valuations of products which might have been produced by other entrepreneurs had they not been displaced. This further conception arises in this way. The money resource input (or " factor prices ") of the first entrepreneur is now supposed to " reflect " these other people's valuations. It reflects these valuations because any one of the factor prices is supposed to be the limit (it should strictly be regarded as being *above* the limit) that the excluded bidder (another entrepreneur) would be willing to pay for the factor in order to produce a product himself, and, accordingly, to reflect its contribution to the value of that product to consumers.[3]

[1] Which, if (1) the entrepreneur were allowed to be considering the production of a second product, besides the alternative of disposing of his resources in the resource markets, and (2) cost were regarded as being the *best* of the rejected alternatives, could be the contemplated revenue from one of the products. Further, if the calculations were marginal calculations (which might incidentally lead to the selection and subsequent production of some of each product, so that the entrepreneur became a " multi-product " entrepreneur), marginal cost could be the contemplated (extra-) marginal revenue from one of the products.

[2] Space does not permit me to enlarge either upon how this replacement might be permissible as a conceivable limiting case, or upon the necessity for rejecting the money input concept on the ground that money is not the entrepreneur's only resource.

[3] My account of the transition from (1) the isolated producer's end-product value, regarded as cost, to (2) the entrepreneur's money input or factor prices, regarded as cost, and regarded also as something which reflects the value of excluded products, is offered as a fair statement of what occurs in Robbins' " Remarks upon Certain Aspects of the Theory of Costs ", section I. I cannot accuse him of actually calling the value of the excluded products a social cost, but the suggestion seems to be there, particularly, perhaps, in his insistence that the excluded products *themselves*, as distinct from their values, are *not* to be regarded as cost. For doubt cast upon the validity of the method of transition from the individual situation to the social situation, see Hayek's discussion in which he stated that he had " long felt that the concept of equilibrium itself and the methods which we employ in pure analysis, have a clear meaning only when confined to the analysis of the

So it now appears that the meaning of cost, in an instruction to an entrepreneur to observe a certain relationship between cost and revenue, could be any one of these three. The very doubt about which meaning to apply would impede the proper implementation of the rule, for, clearly, both controller and controlled would, in this respect, have to be at one. However, this obstacle to the control is supplemented, so as to reduce the idea to absurdity, by the realities that are obscured by the manner of discussion of the conditions of equilibrium.

3. Neglect of Time and Uncertainty in Equilibrium Analysis

The presentation of the conditions of equilibrium[1] proceeds in such manner as to suggest that the act of choice and the complete implementation of it occurred simultaneously. There is no apparent gap in time either between the selection of a course of action with its advance reckoning of cost and revenue, or between the beginning of the actual expenditure of resources and the termination of the achievement of the valued outcomes of the course of action taken. The rational choice which plans the achievement of a " revenue ", by the disposition of resources, at a " cost " in some sense, virtually disappears from view, or becomes merged in the actual disposition of resources and the actual achievement of the actual " revenue ". The termination of the achievement of the valued outcomes (" revenue "), which, in reality, may occur only at the end of a long means-ends chain of events (purchases, conversions into outputs, sales and the like) occurring over a long period of time, appears to be simultaneous with a single resource input. This procedure of telescoping time out of existence tends to obscure two things, which, brought into light, are devastating to any supposition that the rule under criticism could be implemented.

First, it obscures the fact that any cost in the sense of a displaced alternative value or revenue (the imagined outcome of a course of action which is *not* taken) will never have an actual, realised, counter-

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action of a single person, and that we are really passing into a different sphere and silently introducing a new element of altogether different character when we apply it to the explanation of the interaction of a number of different individuals ", and in which he stated that " the data which formed the starting point for the tautological transformations of the Pure Logic of Choice . . . meant . . . only the facts . . . which were present in the mind of the acting person . . . But in the transition from the analysis of the action of an individual to the analysis of the situation in a society the concept [of ' datum '] has undergone an insidious change of meaning " (Hayek, *ibid.*, p. 35 and pp. 38–9 respectively).

[1] My comments in this paragraph are supposed to reflect fairly upon Robbins' discussion of the conditions of equilibrium in the " Remarks upon Certain Aspects of the Theory of Costs ", but are not supposed to suggest that he is oblivious of time and uncertainty, either elsewhere in that paper, or in *The Nature and Significance of Economic Science*. In *The Nature and Significance* uncertainty appears, e.g., in references to the theory of profit. In the " Remarks . . . ", it appears to belong to references to disequilibrium and equilibration (and to criticisms of Marshall), rather than to the discussion of the theory of costs in its competitive equilibrium setting. (And it was surely competitive equilibrium that set the standard for the special rules about the cost/revenue relationship.)

part, observed as achieved results, to compare with the imagined outcome. This absence of an actual counterpart is a necessary condition of the situation, whether we are referring to the entrepreneur's *own* cost, or whether we are referring to the " social " cost[1] (both of which I have explained in section 2 above), and is a condition which would obviously prevent the rule from being implemented in a manner which required *results* to be looked at to see whether the rule had been observed.

The second thing that is obscured and has to be brought to light is something which stands in the way of implementation of the rule, not at the time of the checking of results, but at the earlier time of the generation of the rational choice in the mind of the maker of it, that is to say, at the time when the controller would, presumably, want to know whether the planned course of action was in accordance with the rule. This is the uncertainty, or limited rationality,[2] of the entrepreneur or anybody who tries to supersede or control him. When it is understood that a reckoning of cost, according to any one of the three concepts[3] referred to in section 2, depends upon the forecasting of events and outcomes of the future, and when it is understood that any individual is uniquely situated in relation to past events on which such forecasts are based, it becomes clear that the result of the reckoning is dependent for what it is upon the unique knowledge and attitude (towards uncertainty or risk) of the unique and uniquely situated individual who calculates it, and that the validity, correctness or authoritativeness of an over-riding calculation by somebody else would often be dubious in the extreme. The cost (as well as the revenue) calculation, or residual elements in it, is ultimately a matter of subjective opinion, and, where one person is trying to control the other, is likely to be a matter of differences of opinion. To the extent that this conflict arises and remains, the substitution of one opinion (the controller's) for another (the controlled's) as the authoritative one can be regarded as effecting, also, a transfer of the responsibility for the calculation. The prevailing opinion itself then escapes control, and it, instead of the other one, has to be taken on trust.

I have related these obscurities to the timelessness that appears in the statement of the conditions of equilibrium. This notion of equilibrium was attacked, and a different notion substituted, by F. A. Hayek in 1937.[1] His attack was directed largely against the assumption of perfect knowledge (or what I have referred to as the omniscient type of rationality),[2] and against the use of the propositions of the Pure Logic of Choice (which were supposed to relate to the choice of equilibrium of the single individual) for the purpose of describing an equilibrium (or social choice) of many people working competitively.[3] Thus his attack supports my attitude in this section, and casts doubt on the validity of the use, as the cost concept, of the value to consumers of displaced products that might have been produced by entrepreneurs other than the one having to make the cost calculation—the third concept in section 2. His restatement substituted time[4] for timelessness, and recognised a distinction between the individual's plan (or what I have called rational choice) and the subsequent execution of it. The individual's actions, taken in execution of his plan, were to be regarded as being in equilibrium relationships with one another so long as his actual actions agreed with his planned actions[5]—which means, approximately, so long as the emerging events of the external world, including the actions of other individuals in execution of their own plans, permitted this agreement, or, in other words, so long as the relevant forecasts expressed or implied in his plan proved adequately correct. Similarly, for society, or individuals, at large, equilibrium and its continuance depended upon an adequate compatibility of one another's plans[6] and adequately correct forecasting of emerging objective data.[7]

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[1] And, where the cost referred to in the rule is a *marginal* cost, whether we are referring to either of these or to the money input concept. Cf. " The Ruler ", p. 271.

[2] The significance of it will not be fully realised unless it is seen that the period of time between the rational choice and the achievement of the outcome of the accepted course of action may be, not just a minute or less, but anything between a minute or less and ten years or more. The degree of uncertainty will often be extremely high.

[3] It should be clear that this obstacle to implementation would remain if the meaning of cost were money (or other) resource input. However short the period of time between the cost calculation and the occurrence of the events which were the subject of it, the cost calculation would be uncertain and a matter of subjective opinion. While, in pure equilibrium analysis, " it is simply assumed that the subjective data coincide with the objective facts " (Hayek, *ibid.*, p. 43).

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[1] *Ibid.* And see also his, in many ways, supporting series of articles, " Scientism and the Study of Society ".

[2] It is significant that in his criticism of the pure equilibrium analysis, Hayek remarks: " It seems that that skeleton in our cupboard, the ' economic man ', whom we have exorcised with prayer and fasting, has returned through the back door in the form of a quasi-omniscient individual " (Hayek, " Economics and Knowledge ", p. 44). In his restatement, he said: " It is important to remember that the so-called ' data ' from which we set out in this sort of analysis are (apart from his tastes) all facts given to the person in question, the things as they are known to (or believed by) him to exist, and not in any sense objective facts " (p. 36). The " data ", distinguished from " the objective real facts " as supposed to be known by the omniscient economist (p. 39), are to be conceived of " in the subjective sense, as things known to the persons whose behaviours we try to explain " (p. 39). " Subjective data " and " individual plans " can be used interchangeably (p. 43, footnote).

[3] Cf. footnote 3, p. 150.

[4] " Since equilibrium is a relationship between actions, and since the actions of one person must necessarily take place successively in time, it is obvious that the passage of time is essential to give the concept any meaning " (*ibid.*, p. 36).

[5] " The equilibrium relationship comprises only his actions during the period during which his anticipations prove correct " (*ibid.*, p. 36).

[6] " For a society then we *can* speak of a *state* of equilibrium at a point of time—but it means only that compatibility exists between the different plans which the individuals composing it have made for action in time " (*ibid.*, p. 41).

[7] " It [equilibrium] will continue . . . so long as the external data [the objective real facts] correspond to the common expectations of all the members of society " (*ibid.*, p. 41).

I should like to add here that presumably a firm would begin to *re*plan as soon as it began to *anticipate* that its original anticipations would, for one reason or

4. NEGLECT OF DISCONTINUITY OF KNOWLEDGE WITHIN " THE FIRM "

I referred, in section 2, to the apparent conversion of the individual cost/revenue relationship into a social cost/revenue relationship, and, in section 3, to (what amounts to much the same thing) the conversion of the individual choice or equilibrium into a competitive social equilibrium. An objection to the latter conversion, and hence to the former, is that, in this competitive social situation, there is discontinuity of knowledge, as between the different individuals who, each on the basis of his own particular knowledge, make the forecasts on which, in turn, their own individual actions are based. Hayek, in raising this objection with regard to the competitive situation in particular, was pointing[1] to the lack of theories relating generally to the communication of knowledge. Regrettably, although many of his general observations were relevant, his interest did not lie specifically in the similar discontinuity of knowledge inside large business organisations. Consequently, he was not led to criticise the practice, in the theory of the firm, of drawing no distinction between the one-man firm and the multi-man firm—a practice which ignores the possibility that, in the business organisation, there might be any number of planning individuals ranging from two to as many as would be found in the competitive situation.

The discontinuity of knowledge inside the organisation is significant, in the context of economic and administrative theory, for at least two reasons. First, it raises the question whether the conversion of the idea of an individual cost/revenue relationship into the idea of an overall, or organisational, cost/revenue relationship is worthy of the same scepticism as is the conversion of the idea of an individual cost/revenue relationship into the idea of a social cost/revenue relationship. Secondly, it relates to the question whether an internal rule relating to the cost/revenue relationship is non-implementable in the same way as is an externally imposed rule. On this second question, I do not propose to say much more than that what applies to a rule supposed to be imposed by an external controller upon the entrepreneur applies in much the same way to a rule supposed to be

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another, be falsified—and then act according to the revised plan. So perhaps we can speak of its being out of equilibrium with respect to its old plan and in equilibrium with respect to its revised plan. With this adjustment, the Hayekian notions here expressed supply an amenable milieu or containing theory—for my own view that the firm's *account* (which I regard as a counterpart statement of realised or actual events) always agrees, or ought to agree, with its *revised* budget (which I regard as its statement of anticipated events, or its plan) though not necessarily with its earlier, or original, budget. See my " The Ruler ", p. 270, footnote 39, and my " Notes on the Maximisation Process in Company Administration ", *Economica*, August, 1950, pp. 268-9 (particularly footnotes) and p. 278. And see the connection between my view and the modern question of what plans of the directors of large organisations should be submitted to the stockholders before they are put into operation (" Notes on the Maximisation Process in Company Administration ", pp 276–7, particularly footnotes).

[1] In both " Economics and Knowledge " and " Scientism and the Study of Society ".

imposed by a superior (co-ordinating) administrator upon a subordinate administrator in the organisation. I want to deal briefly with the first question, because I do not wish to leave the impression that if the social relationship goes, the organisational relationship goes with it. Before I do that, it will be well to look at what is left of the cost/revenue relationship after my treatment of it in sections 1 to 3.

It will be seen that, after rejecting the other notions of cost, I am left with the view that cost is the entrepreneur's own second-best outcome-value (second-best " revenue ") reckoned by him according to his own uncertain, highly subjective, calculations, and rejected by him in favour of his own, similarly calculated, best outcome-value (best " revenue ") from the use of the same resources. I am left, too, with the view that, with the multi-product and output-variation ideas introduced, this best outcome-value differs from the second-best outcome-value only by a *net* marginal variation. I say " *net* " because the marginal variation in the planned course of action, upon which the change in value is dependent, can be conceived of as having a minus (" marginal cost ") effect and a plus (" marginal revenue ") effect upon the value—e.g., when the variation in the course of action consists in ideationally reducing the output of one product (and so its revenue) a little, and increasing the output of another product (and so its revenue) a little.[1]

This view can now be reduced to the simple statement that the entrepreneur's total cost is approximately equal to his total revenue,[2] and his marginal cost is approximately equal to his marginal revenue,[3] but it is a view which allows *the planned revenue, related to the planned resource input (money or mixed) of the selected course of action, to be of any magnitude.*[4] And, to reiterate, it is a view which sees the costs and revenues as subjective data—unrealised planning data belonging to the decisional process preceding action.[5]

Now, it appears to me that a rule, based on this theoretical statement of the final situation in the entrepreneur's decisional process, would be not only non-implementable, but also about as instructive and guiding as—and possibly much less intelligible than—an injunction of the order of " seize your best opportunities ! ". The statement is not intended to be the basis of a rule to instruct or control the behaving subject: it is rather a theoretical, hypothetical, statement offered as a description of the behaviour of the behaving subject.

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[1] I hope to attempt, in a subsequent paper, a reconciliation of this restatement, and some other aspects of my paper, with certain modern administrative theory.

[2] That total cost (in some sense) should be equal to total revenue is one of the rules (or part of one).

[3] There is a rule (or part of one) that where marginal revenue differs from price, output should be extended to that point at which marginal cost (in some sense) is equal to price. This rule, I believe, falls away with the " perfect competition " which is the ground on which it is based. However, with cost defined as I have defined it, it appears to fall away for another reason, too. With cost so defined, marginal cost is always displaced alternative marginal revenue, and, consequently, from the point of view of the rule advocates, open to the same stricture as is the selected marginal revenue itself: to get a marginal cost which was independent of autonomous pricing, we should be driven back on to marginal money input.

[4] I.e., there is no " cost "/revenue equality in this sense.

[5] The statements in this paragraph and the preceding one represent the principles behind the model (in which, however, the entrepreneur is converted into an organisation) in my " The Subjective Theory of Value and Accounting ' Cost ' ". See next footnote.

I shall now assume that this statement, understood as I have explained it, is as applicable to the organisation as to the individual, and that the multi-person organisation replaces the one-man entrepreneur as the behaving subject.[1] And I turn to the question whether this conversion is as worthy of criticism as is the conversion of the individual cost/revenue relationship into the competitive social cost/revenue relationship. On this issue, and in support of the idea of the organisational cost/revenue relationship, I can do little more here than point to two significant differences between the competitive situation and the organisation:

(1) In the organisation, the individuals are assumed to be common-goal-oriented.[2] Hence, the organisation itself, although it has no mind, has to be viewed anthropomorphically,[3] that is to say, *as if* it were behaving rationally, like a man, and has consequently to be assumed, in determining its course of action, to develop an overall, or organisational, cost/revenue relationship. In the competitive situation, on the other hand, the individuals are not assumed to be common-goal-oriented in this way. Consequently, the overall (social) cost/revenue relationship is not implied in this way.

(2) In the organisation, the individual decision-makers—the sectional planning administrators—are assumed to declare their plans, for the purpose of interpersonal co-ordination, before their plans are executed. This behaviour is an essential condition for the acceptance of the view that there is an overall, or organisational, cost/revenue relationship: the relationship is a reflection of this declaration and co-ordination.[4] In the competitive situation, on the other hand, this *ex ante* declaration and co-ordination does not occur.

There is now little more that can be done, in the space available, than to stress that this assumption that the individual declares his plan for the purpose of co-ordination does not imply that the coordinator perceives and comprehends it with the same exhaustiveness as does the individual who submits it, or that the individual perceives and comprehends the other plans, with which his own have to be co-ordinated, with the same exhaustiveness as do the other individuals who have to prepare them. In the organisation there is discontinuity of knowledge between people of limited rationality, gaps that are filled by reciprocal (mutual) authority relationships which replace some of the single-minded co-ordinations which occur when the individual operates alone in a narrower sphere. The individual's plan, though it clearly must disclose the elements which are required to be known for the purpose of co-ordination,[1] is, as communicated, an authoritative statement whose acceptance is, of necessity, one which is, to a greater or less extent, accepted, on trust, as a communication possessing the potentiality of reasoned elaboration.[2] To say this is not to say either that the reasoned elaboration is never requested, or that the plan can never be " faked "—any more than that these same assertions would be made of authoritative communications in other spheres. But acceptance on trust has to enter. And within what is so accepted will often be much revealed and unrevealed cost and revenue calculation.

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[1] This is the conversion that I effected in " The Subjective Theory of Value and Accounting ' Cost ' ". See preceding footnote.

[2] It seems to me that common-goal-orientation is silently—and perhaps unconsciously—assumed in the economists' practice of using the same cost/revenue diagrammatic apparatus to represent the behaviour of " the firm " regardless of whether the firm is a one-man or a multi-man set-up.

[3] Hayek, condemning " an illegitimate use of anthropomorphic concepts ", adds, in a footnote, " What is said in the text does of course not preclude the possibility that our study of the way in which individual minds interact may reveal to us a structure which operates in some respects similarly to the individual mind " (Hayek, " Scientism and the Study of Society ", Part II, p. 45 and footnote).

[4] The process is illustrated in my " The Subjective Theory of Value and Accounting ' Cost ' ".

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[1] For an illustration of what elements would have to be disclosed, see, again, the model in my " The Subjective Theory of Value and Accounting ' Cost ' ".

[2] For this expression, see Carl J. Friedrich, " Authority, Reason, and Discretion ", in *Authority*, ed. Carl J. Friedrich, 1958.

Methodological Prescriptions in Economics: A Reply

By T. W. HUTCHISON

In a recent issue of *Economica*,[1] Messrs. Klappholz and Agassi direct expert and intricate criticism against some definitions and classifications in a book of mine of 1938[2] to the effect that, misled by a false dichotomy between types of statement, I was guilty of the " imposition of my own meaning on other people's writings " (p. 64). It may very well be that the dichotomy I employed is far from satisfactory in itself and in the way it was applied, but the two examples Messrs. K. and A. give of the misleading effects of this dichotomy seem devoid of foundation.

1. Messrs. K. and A.'s first example concerns the treatment of statements containing unspecified *ceteris paribus* clauses.

They assert:

(i) That I argued that " statements with unspecified *ceteris paribus* clauses . . are tautological " (p. 63); and (ii) that I further insisted on " viewing as tautological *all* statements of economics which are qualified by unspecified *ceteris paribus* clauses " (p. 64, italics added). Messrs. K. and A's assertions are unsupported by any references or quotations. In fact, it was stated in my book:

(i) that such propositions " are *frequently* treated as analytical-tautological propositions " (p. 41, italics added—i.e. not even usually, and certainly not " always ");

(ii) that " perhaps such propositions are sometimes meant in one way, sometimes in another " (p. 40);

(iii) that " either of these interpretations [i.e. empirical and testable *or* tautological] is possible *and there may be others* " (p. 45, italics added); and finally

(iv) that, " *ceteris paribus* propositions are frequently hopelessly ambiguous " (p. 162). Thus, however grave its deficiencies, one thing my dichotomy did *not* do was to lead me to view " as tautological *all* statements of economics which are qualified by unspecified *ceteris paribus* clauses " (italics added), but only as " frequently "—or as Messrs. K. and A. themselves apparently hold —" sometimes " tautological. I would add that it seems to me that if the content of " ceteris paribus " is reasonably specified or implied, a statement containing that qualification would be empirical and testable. If not, it is " hopelessly ambiguous " and it seems trivial to say that such a dichotomy cannot account for the admittedly " hopelessly ambiguous."

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[1] " Methodological Prescriptions in Economics ", *Economica*, February 1959, p. 60.

[2] *The Significance and Basic Postulates of Economic Theory*, 1938.

2. Messrs. K. and A's second example of the malign influence of my false dichotomy leading to " H's imposition of his own meaning on other people's writings " is exemplified in the statement in my book: " With regard to prognoses also we find empirical content being read into propositions of pure theory just because of their necessity and inevitability " (p. 65). This comment referred, according to the note (38) attached to the succeeding sentence, to a passage on p. 111 of the first edition of Professor Robbins' *The Nature and Significance of Economic Science* (repeated on p. 122 of the second edition), *which I summarised almost verbatim in that succeeding sentence.* Messrs. K. and A. discuss p. 117 of Robbins' book (which is not referred to by me explicitly or implicitly). They insist that my statement deals unjustly with a claim of Robbins about the quantity theory of money (attributing to him the view that its " necessity " is that of " a verified empirical law "—a surprising conception for at least three reasons). *But Robbins did not mention the quantity theory of money within several pages of the single precise reference I gave and was discussing.* His nearest example comes from the field of taxation. I was simply concerned to comment on a general statement in general terms. Thus Messrs. K. and A's three comments regarding whether Robbins was employing Sir William Petty's version of the quantity theory or some other, etc., etc., are quite irrelevant for establishing their example. Far from imposing any meaning on Robbins' interpretation of the quantity theory, I was not actually discussing his interpretation at all. My comment still seems justified, quite certainly in the context of Robbins' first edition and not improbably in that of his second. However, if Messrs. K. & A. feel that my comment was on or over the margin of justifiability, let them show it. But they will first have to sort out their contexts and references. Meanwhile, as Messrs. K. and A. aptly put it, " the reading of content into a tautology . . . robs it of its certitude " (p. 64). And it seemed and still seems that Robbins was not really facing up to having his economic laws thus robbed.

Messrs. K. and A. appear to agree that Robbins must *either* (*a*) have been reading empirical content into tautologies, or (*b*) reading analytical-tautological certainty into empirical statements—equally criticisable procedures. If they wish to insist that it was (*b*) and not (*a*) in which Robbins was indulging, I would not want to argue at length that it was (*a*). Either charge implies the same basic criticism, and it does not seem that one is any more " imposing one's own meaning " on Robbins—and on the evidence rather less so—if one holds that (*a*) seems a more probable interpretation than (*b*).

3. Messrs. K. and A. also make one or two generalisations: " Often he criticised economists for uttering tautologies when their utterances were not tautologies except on his interpretation " (p. 64). Whether or not the adverb " often " can be substantiated at all, Messrs. K. and A. have certainly not produced a single supporting case so far.

It may be that they are referring to pp. 55–7 of my book where a mixed array of propositions or doctrines was shown by quotation to have been sometimes interpreted, *not by me but by other people*, very distinguished economists, as tautological. Incidentally, I might have quoted in this passage Professor Robbins' reference elsewhere to "a tendency in some quarters to revert to more mechanical forms of the Quantity Theory, in particular, to proceed by way of a multiplication of purely tautological formulæ".[1] Presumably, a meaning was being imposed on somebody there?

University of Birmingham.

A Rejoinder

K. Klappholz and J. Agassi

1. Before we attempt to answer Professor Hutchison's strictures on our comments on his book, may we briefly restate our main point. Our paper was primarily a criticism of an attitude of impatience and haste which seems prevalent in methodological writings on economics. It seems to us that Professor Hutchison's dichotomy—his view that all statements must be either tautological or testable—provides support for this attitude, since it justifies the impatient dismissal of ideas which are not yet testable (but which may become testable by further specification). If the dichotomy were correct, such untestable ideas necessarily would be tautologies and uninformative.

2. We sought to refute the dichotomy by providing counter-examples to it. These consisted of statements qualified by *unspecified ceteris paribus* clauses. We claimed that such statements are untestable. For, in order to refute them empirically, we must observe the variations of the factors which are explicitly mentioned in them while observing the constancy of *all* the other factors in the universe of discourse. "*Ceteris paribus*, the imposition of a tax on cigarettes will raise their price", and, "*Ceteris paribus*, the imposition of a tax on cigarettes will *not* raise their price", are untestable statements as long as the *ceteris paribus* clauses in them remain unspecified. However, they are not both true, since they are incompatible with each other.

Professor Hutchison does not deny that statements qualified by unspecified *ceteris paribus* clauses are untestable; nor does he deny that it follows from this and from his dichotomy that such statements must be tautological. Yet he criticises us for attributing this conclusion to him (when we referred to chapter 2, section 5 of his book).[2] It seems to us that Professor Hutchison now implicity concedes that his dichotomy was inadequate when he says that an ambiguous statement may be neither tautological nor testable; which is all we endeavoured to show in this context.

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[1] Introduction to L. Mises, *The Theory of Money and Credit*, 1934, p. 13.

[2] In view of his dichotomy we do not know what "other" interpretations (i.e. other than tautological or empirical) Professor Hutchison had in mind.

3. We regret our mistake in thinking that Professor Hutchison was referring to Professor Robbins' discussion of the quantity theory of money. On. p. 65 of his book Professor Hutchison complained that economists often read content " into propositions of pure theory "; he appended a footnote referring to Robbins and then continued in the text with a discussion of the quantity theory of money. We could not see the relevance of the reference, and thought that it must have been erroneous and that Professor Hutchison had wished to refer to Robbins' discussion of the quantity theory. Now that our mistake has been pointed out we must insist that the particular passage in Robbins, to which Professor Hutchison in fact referred, does not seem to us to be an example of " empirical content being read into " tautologies. Admittedly, Robbins held that economic laws described " inevitable (or certain) implications ". Professor Hutchison now suggests that Robbins could be criticised either for " reading empirical content into tautologies " or for " reading analytical-tautological certainty into empirical statements ". If he means that Robbins was mistaken in claiming that one and the same statement can be both empirical and certain, then this is what we said in our paper. But we cannot agree that it is " the same basic criticism " to say that someone views a tautology as empirical *or* that he views an empirical hypothesis as certain. A statement is not rendered tautological simply by the claim that it is certain; whether or not a statement is logically true is not quite so arbitrary. Once the meaning of a statement is determined its logical status follows. Robbins' understanding of " economic laws " makes it impossible to conclude that they are tautologies, since on his interpretation " economic laws " can be used as bases for prediction. (This was the point of our discussing his view of the quantity theory of money.)

Moreover, stating a tautology as if it were empirical is playing safe without being informative, while stating a hypothesis is informative, in spite of a possible claim that it is certain. The claim that some hypotheses were certain was common in the history of physics, while the habit of presenting tautologies as if they were informative is more peculiar to some social sciences. Professor Robbins' error is in the first tradition rather than in the second.

4. We have no doubt that economists often mistook logic for fact, that many of Professor Hutchison's strictures were justified, and that a methodological criticism was urgently needed. But while we are in sympathy with the broad intentions of his criticism and have great respect for his contribution, we regret to find some of Professor Hutchison's basic tenets unacceptable and believe them to give unjustifiable support to the kind of impatience we sought to criticise.

The London School of Economics.

The New Age of Technology, 1750—1900

By D. C. Coleman

The Belief in Technology is to the mish-mash religion of the world of modern industrialism what the Creed is to Christianity. We do not yet, it is true, repeat, as the faithful in *Brave New World* might have repeated: " I believe in Ford the Father ". Perhaps we have no need to: we take this Creed for granted. We may no longer believe in the progress of humanity, in perfectability in the Victorian manner, but, at least in the current boom-world, we all love our engineers. The position of Technology in the gentlemanly education of the Establishment may not yet be assured, but the temples of Technology grow apace. The new Oxford *History of Technology*[1] is a literary temple. It is High Church. With the appearance of Vols. IV and V[2] the great edifice is complete.

What is it for? According to the preface of Vol. I it was to be: " A history of how things have been done or made ", thus to serve " as an auxiliary to the more specialist studies of historians, archæologists and other scholars ".[3] So it may legitimately be criticised by the criteria used to judge a work of reference. Certainly no economic historian can afford the luxury of ignorance about the growth and impact of technical advances through the ages. Does the *History* as a whole succeed, then, as a reference work such as the economic historian, *inter alia*, might wish to consult?

There are certain idiosyncracies of coverage[4] though in general the width of learning is formidable. But it has one serious practical defect. The varying distribution of topics and chapters, chronologically or by subject-matter, often makes relevant information difficult of access. One might suppose, for example, that the great achievements of Abraham Darby and Thomas Newcomen would be considered in Vol. III, covering the period 1500–1750. In fact, Newcomen's name does not even appear in the index to that volume (though it is mentioned briefly in the text); the work of both men is dealt with in Vol. IV which, by covering the period 1750–1850, thus starts at a date well after both of them were dead. Or again, " Road policy, 1600–1775 " is part of a chapter, in itself most informative, which appears in the 1750–1850 volume, and is entitled " Roads to c. 1900 ".[5] But should anyone inquisitive about this topic in the seventeenth century consult the seemingly relevant volume, III, he will find no indication whatever of the existence of this information in Vol. IV; he will instead find roads dealt with in a totally different context in a chapter called " Town-Planning from the Ancient World to the Renaissance ",[1] most of which has nothing to do with the period allotted to Vol. III. And for a final example, if our hypothetical investigator should seek light on the mechanization of printing and papermaking in what would seem to be the appropriate volume, IV, he will find nothing: although in both industries that mechanization took place between 1800 and 1850, the former has to wait upon Vol. V, which covers the half-century 1850–1900, and the latter has already secured its brief mention in Vol. III.

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[1] *A History of Technology*, 5 vols., ed. Charles Singer, E. J. Holmyard, A. R. Hall and Trevor I. Williams (Clarendon Press, Oxford, 1954–58, £8 8s. per volume).

[2] *Ibid.*, Vol. IV, *The Industrial Revolution, c. 1750—c. 1850*, and Vol. V, *The Late Nineteenth Century, c. 1850—c. 1900.*

[3] Vol. I, p. v.

[4] See below, p. 168.

[5] By R. J. Forbes.

The editors show their awareness of some of these problems in sundry prefatory observations. But these are of little help to the user of a multi-volumed reference work. Much more helpful would be an index volume covering the entire *History*. As it is, each volume has its own index but the sole indicator of the total contents is a list of all chapter headings which lurks in an unexpectedly foreign fashion at the end of Vol. V. This is an inadequate guide, particularly in view of the method of arrangement which has been followed; and without an adequate guide, such a reference work cannot be completely successful.

To judge these splendidly produced volumes in this manner may seem to be judging them as though they constituted, or were intended to constitute, an encyclopædia. And in some ways this monument to the Belief in Technology is in that respectable, though scarcely hallowed, tradition of encyclopædias and dictionaries of " Arts and Manufactures ". Can it also be a history in the sense in which history is something more than a chronicle? To push this question further: is there such a thing as " the history of technology ", *per se*? Ford said that history was bunk. Is a history of technology bunk?

" A history of how things have been done or made ". So far so good. But Clio today demands to know " why " as well as " how ", and for this the limits of technology are too confining. Technological history in the narrowest sense is as repellant and meaningless as are some of the petty specialisms into which the study of history is already disintegrating. Again, the editors of the *History* show themselves to be aware of this problem. They have tried to meet it by the provision of various chapters, distributed over the volumes and designed to provide links with the outer, non-technological world. It is not enough. Ultimately, technology itself and the massive size of the whole undertaking, defeat their best intentions. The shortcomings of an approach which makes few consistent efforts to reconcile the " how " of technology with the " why " of history become increasingly evident in the later volumes. The earlier volumes cover many centuries during which techniques were simple and for which detailed evidence is scanty; and in those volumes a fair degree of success has been achieved in demonstrating the interplay between man and his environment in his efforts to master natural forces, in showing the relationship between primary economic problems and the evolution of techniques. But as the periods of time covered by the volumes grow shorter, as the techniques become more complex and evidence more plentiful, so do those relationships become harder to discern. Some contributors have attempted, with varying degrees of success, to illustrate them; others seem to have made hardly any effort to do so.

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[1] By Martin S. Briggs.

So by neither of these criteria is the *History* really successful. Whilst trying to be an encyclopædia or dictionary of technology, it is an unsatisfactory history; and in aiming at history, it is not always an adequate encyclopædia. Nevertheless, the *History* is still an impressive work in its range and erudition. There is much in it that is valuable, informative, enlightening and interesting; there are numerous useful and some splendid illustrations; though its quality is uneven there is much of a very high order; and, for all its defects, it is probably the best reference work on its subject.

How, then, do Vols. IV and V of this " auxiliary " answer some of the questions which an economic historian might wish to put to them?

For some time past the British Industrial Revolution has been shrouded in the fog of battle, the battle about the standards of life of the workers of the time. Through the smoke and above the din comes an occasional testimony to a smaller and less exciting difference of opinion. It is one about which an appeal may be most aptly made to the last volumes of the *History*. What was the role of science in the Industrial Revolution? How much was it a matter of empirical know-how, of inspired guesses by rude mechanics, of blacksmiths with genius, and how much of the application of scientific thought, of principles derived from experiment, hypothesis and observation?

In the earlier phases, science apparently played only a trifling part: . . . " the new machinery in the textile industry involved no principles that an intelligent merchant could not grasp. Indeed, much of the mechanical invention of these years was simply the result of the harnessing of the traditional skills of clockmakers, millwrights, blacksmiths, and the like to the needs of the entrepreneur ".[1] The turn of applied science is seen, as coming later, in the second half of the nineteenth century: " the importance of applied science is the outstanding theme of this volume [V], for few branches of manufacture or production were left unchanged by the fruits of scientific discovery ".[2] Was it all quite so straightforward, however, as these seemingly unexceptionable remarks imply? In an important chapter of Vol. IV, entitled " The Beginnings of the Change from Craft Mystery to Science as a Basis for Technology ", A. R. J. P. Ubbelohde emphasizes that the mid-seventeenth century was the crucial period when this "shift from craft mysteries to science" commenced; moreover, thenceforward, "entirely new technologies, without any craft precursors, were developed".[1] And in contemplating the line of thought and action which includes Morland, Huygens, Papin, Savery, Newcomen, Black, and Watt, and which before the 18th century was out had led to the supremely important triumph of steam power, it is hard not to think that the contributions of smiths and wrights, important though they were, can be too easily overrated or assumed to be unscientific. The work of Darby or Hargreaves or Arkwright owed, it is true, little or nothing to science. On the other hand, the creation, by the 1830's, of a recognisable chemical industry would have been impossible without the long roots of scientific thought, stretching back through Priestley and Lavoisier to Boyle. With the practical application to industry in general of such discoveries as those of Scheele and Leblanc we are already a long way from traditional skills, and nearer to that world of modern science the achievement of which we allow to dazzle us only too easily. As E. J. Holmyard brings out with admirable clarity it is upon the basic advances in chemistry, mainly made before 1850, that the modern chemical industry rests.[2]

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[1] Charles Wilson in Vol. V, pp. 799-800.

[2] Preface, Vol. V, p. vi.

If attention is confined largely to the earliest phases of the Industrial Revolution and, in particular, to the undoubtedly striking achievements in the cotton, iron and pottery industries, then there can be little doubt that the role of the new science will seem to have been small. But the full significance of the Industrial Revolution cannot be measured by reference to a few industries, any more than its span can be contained within a few decades; it did not come to an end, like a political revolution, in 1830 or 1850. And it is clear that even during the first half of the nineteenth century the cumulative and pervasive process of industrialization was more and more receiving the stamp of scientific thought: in precision mechanics and in machine tools, especially with the work of Whitworth without which there could have been little advance beyond the first crude machines; in the new gas industry as well as in chemicals; in the application to telegraphy of the growing knowledge of electricity and magnetism, so that without the work of Galvani, Ampère, Volta and Oersted the success of Cooke and Wheatstone in 1837 would not have been possible. Moreover, in many of those branches of industrial technology which we normally think of as peculiarly belonging to the world of the later nineteenth century and thereafter, the fundamental advances were made earlier, though their more spectacular applications waited upon changes in the economic climate. The oil industry, in all fantastic and extravagant ramifications, has its roots in the pioneer work, in the 1840's and 1850's, of James Young and Abraham Gessner in Britain and, in the

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[1] Vol. IV, pp. 668, 671.

[2] Vol IV, pp. 214–29; see also the observations, by the same author, in Vol. V, pp. 267–68.

U.S.A., of the enterprise of the industrialist, George Bissell, and the Yale professor of Chemistry, Benjamin Silliman; and this small flower had effectively begun its life in the 1850's, though it was not until later that a hotter economic sun drew out such luxuriant blooms as a Rockefeller or a Deterding. Though the development of refrigeration is something which we normally, and rightly, regard as beginning in the 1870's and 1880's, the technique of refrigerating machines, working either by an air-compression process or by the evaporation of volatile fluids, was worked out between the 1830's and 1850's. Examples could be multiplied from other industries.

We are accustomed, too, to think of early nineteenth century industrialization as peculiarly British. In its effective application it was. But we must beware of too great a concentration on the deeds of that beloved British character, the "practical man". Many who were neither British nor uniquely "practical" contributed more to the technology of early industrialization in Britain and elsewhere than they are normally given credit for in our rather insular history books. The self-helping heroes of Smiles' sagas of invention and engineering loom large in the standard iconography of British industrial history. Their achievements were real, but so in a different way were those of other men who contributed variously to the progress of nineteenth century industrialization. We hear about Cooke and Wheatstone but not perhaps about the important pioneer work of S. T. von Sömmering and Baron Schilling in Munich some two decades earlier. Oliver Evans may easily elude us, working in America, at the same time as Trevithick, on the high-pressure steam engine and locomotive. Do we always give due credit to the importance, albeit indirect, for industrialization of the scientific work of such men as Dalton, Priestley and Black or later of Davy and Faraday, or of Hans Christian Oersted, Danish chemist and professor at Copenhagen, whose discoveries influenced the technology of electricity, chemistry and metallurgy? In France the pioneer efforts in steam propulsion of the Marquis de Jouffroy d'Abbans or of Nicolas Cugnot did not go unattended with success, though their chances of commercial development in the France of the time were as slight as were those, in other fields, of Nicolas-Louis Robert and of Leblanc, both of whose inventions had to find their way to the bounding British economy for practical development.

Vols. IV and V of the *History* provide some much-needed illumination of many of these pertinent but rather neglected aspects of historical technology. Economic historians would do well to study such valuable chapters as, *inter alia*, those on the chemical industry by E. J. Holmyard and A. and N. L. Clow in Vol. IV, and by Holmyard and his fellow-editor, Trevor I. Williams, and J. McGrath in Vol. V; those, in Vol. IV, on the gas industry by Sir Arthur Elton, on precision mechanics and on machine tools by Maurice Daumas and K. R. Gilbert respectively (the latter chapter contains an instructive diagram showing the successive employer-employee relationships through several decades of leading British engineers), on telegraphy by G. R. M. Garratt, as well as the chapter, mentioned earlier, by A. R. J. P. Ubbelohde; and, in Vol. V, R. J. Forbes on petroleum, and the two chapters, both by C. Mackechnie Jarvis, on electricity.

These are a few of the contributions which impressed the present writer as being particularly interesting and valuable. This is not to exclude others from that category. To consider here all the chapters of these two bulky volumes is impossible—and were it possible, would be immeasurably tedious. Yet, in reading through many of them, there is no escaping the unevenness of quality, the variability of range, interest and approach which was apparent in, for instance, Vol. III.[1] A few examples must suffice.

In Vol. IV, H. R. Schubert's chapter on iron and steel is very much a technician's contribution: one process succeeds another, each is described usefully, precisely, drearily. But why, O why, in so rigidly technical a world as this implies, did any human creature ever do anything? In the several chapters which he has contributed to the work as a whole, R. J. Forbes admirably demonstrates that erudition and a grasp of technical detail need not be indissolubly wedded to stodgy dullness. But though he is always readable and often illuminating, his sallies into the dangerous twilight of economic generalization are not invariably felicitous. His chapter about roads[2] is wide-ranging and valuable; that on power[3] contains much to interest the economic historian, though that historian is not likely to be happy about such confident over-simplifications as this: " economically the basic factor in the industrial revolution was the remarkable expansion of overseas trade in the seventeenth and eighteenth centuries. The new markets came first, the inventions followed ".[4] Nor is this a view which seems to commend itself to all his fellow contributors.[5] In the same chapter he makes the important point that the average capacity of the Watt engine by 1800 was not significantly higher than that of wind- or water-mills. In a table which follows he gives the " duty " and percentage thermal efficiency of various steam engines from Newcomen's in 1718 to a triple expansion engine of 1906. From the data thus given he concludes that " the rapid rise in thermal efficiency in the early nineteenth century is remarkable ". But the figures would hardly seem to support this conclusion, for they indicate an increase of 8-fold in the 74 years from 1718 to 1792 and of rather less than 4-fold in the 86 years from 1792 to 1878.[6]

The variegated pattern continues in Vol. V. T. N. Morris contributes a useful chapter on " The Management and Preservation of Food " which delves into a variety of corners too little explored by the social and economic historian. The survey of steelmaking, again by H. R. Schubert, seems to verge, for so vitally important a topic, on a bare adequacy. And is it by any means so certain that Britain's reduced share in total iron production in the later nineteenth century was " in no way a result of technical inferiority " ?[1] Against this volume's valuable chapters on petroleum, chemicals, and electricity, as mentioned above, there must be set the badly arranged piece of railway antiquarianism which is C. Hamilton Ellis' chapter called " The Development of Railway Engineering ". The railways have fared badly in the *History*. Mentioned only *en passant* in Vol. IV, to which chronologically their first eruption belongs, they merit a more comprehensive, integrated and analytical treatment than they finally get in Vol. V. In his contribution in this volume to the history of building materials and techniques, S. B. Hamilton seems much more at home than in his chapters in earlier volumes. There are a number of matters here which economic and social historians, who so often neglect the building industry, would do well to consider: the relationship, for example, between the " balloon frame " house and the machine-forged nail.

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[1] Cf. my review of Vol. III: " Technology and Economic History 1500–1750 ", *Economic History Review*, 2nd Series, Vol. XI, April, 1959, pp. 506–14.

[2] " Roads to c. 1900 ", in Vol. IV.

[3] "Power to 1850 ", also in Vol. IV.

[4] Vol. IV, p. 148.

[5] Cf. E. J. Holmyard, Vol. V, pp. 267–68.

[6] Vol. IV, pp. 163, 164.

Arrangement and choice of material, and the coverage afforded thereto are not always ideal. The three chapters[2] which constitute Part II of Vol. IV, entitled " Forms of Energy ", contain a good deal of overlapping and repetition, and some hark back to earlier periods. The gigantic water wheel at Marly, for example, built for Louis XIV in 1682, which is discussed and illustrated here, seems to belong in more ways than one to the world of Vol. III where in fact, however, there is no mention of it at all. In Vol. V, Part VII is entitled " Manufacture " : it includes textiles but not chemicals (in Vol. IV the chemical industry was included under " Manufactures "); it covers the working of metals but not the manufacture of steel. Chap. 25 (R. Chadwick, " The Working of Metals ") is thus oddly placed in relation to Chaps. 3 and 4 (" The Steel Industry ", by Schubert, and " New Extraction Processes for Metals ", also by Chadwick), so that the reader will find a sub-section of Schubert's chapter dealing with rolling mills on pp. 63–64, and a more detailed account of them on pp. 611–20. If the treatment given to railways in the *History* is poor, that allotted to the paper industry is miserly. To the unsatisfactory arrangement and deficient coverage in the earlier volumes,[3] Vol. V adds no more than a paragraph in the course of W. Turner Berry's chapter on " Printing and Related Trades ". This gives an entirely inadequate account of the development of wood-pulp, a technical advance of the later nineteenth century, which has had very far-reaching consequences.

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[1] Vol. V, p. 67.

[2] R. J. Forbes, " Power to 1850 "; H. W. Dickinson, " The Steam Engine to 1830 "; and A. Stowers, " Watermills c. 1500—1850 ".

[3] A meagre appendix to the chapter on printing in Vol. III, attempting to cover the whole history of the manufacture up to and including mechanization in the nineteenth century.

Vol. V, and the whole *History*, concludes with three chapters which together comprise Part VIII, resoundingly entitled " The Threshold of the Twentieth Century ".

The first is by Sir Eric Ashby: " Education for an Age of Technology ". It tells the cautionary tale of the attempts in the nineteenth century to establish technical and scientific education in this country, contrasting them, though rather briefly, with achievements elsewhere. England resisted. " It is plainly out of the question ", said the headmaster of Winchester in the 1860's, " that we should teach chemistry ".[1] There seem to be some concealed ironies in this chapter. Noting that University College, London, early had chairs in science and engineering, Sir Eric observes that there therefore existed " an opportunity as long ago as 1837 (when University College was incorporated in a University of London) for managers and industrialists in England to give their sons a balanced and liberal education in science ".[2] Then, only a very few took that opportunity. And today? Have today's managers and industrialists turned their back on the lure of Oxbridge for their sons, in favour of London's attractions? " The hierarchical nature of English society sheltered the upper classes from all educational influences except those prevailing at the public schools and the older universities ".[3] Thus Sir Eric on the high noon of later Victorian England. And today? Are today's upper classes exposed to the withering winds of state schools and Redbrick?

Charles Wilson contributes the second chapter, " Technology and Industrial Civilization ". It is lucid, well balanced, useful and . . . discreet. " Industry in 1900 ", he comments, " meant many things it had not meant in 1875, including Sunlight soap, margarine, cheap jam and Bovril ".[4] It is ironic rather than indiscreet that not one of these commodities is mentioned elsewhere in the volume.

And so to the final chapter—Sir Alexander Fleck, " Technology and its Social Consequences ". Without the generosity of Imperial Chemical Industries the *History* would presumably not have been possible. It is neither surprising nor unreasonable that chemistry and the chemical industry should have fared very well in the *History*: the chapters on chemical topics are amongst the best in the whole work; of the four editors, two, Holmyard and Williams, are chemists with scholarly interests in the history of chemistry; and that subject has also figured largely in the eminent researches of a third, Charles Singer. So it is again neither surprising nor unreasonable that the closing chapter should appear under the name of the then Chairman of I.C.I.

Unfortunately, it is not a very distinguished ending to the work, for the chapter is a curious hotch-potch of trenchant remarks, dubious generalizations and simple inaccuracies. Here are a few examples.

[1] Vol. V, p. 785.
[2] *Ibid.*, p. 781.
[3] *Ibid.*, p. 786.
[4] *Ibid.*, p. 806.

" As timber resources dwindled, the charcoal furnaces of Sussex iron-smelters could not compete with the coal-based iron industry of South Wales " (p. 816). In fact, they were ceasing to compete, and many of them had disappeared, well before there was any coal-based iron industry in South Wales; and they went down not in the face of a competing coal-based iron industry or because of dwindling timber resources, but because imported Swedish and Russian charcoal-iron was cheaper. " Many of the workers who moved into the new urban areas were small farmers, agricultural labourers, or handloom weavers who had been forced by enclosures, the " Speenhamland Act ", or the advent of textile machinery to seek a livelihood in the towns " (p. 816). There was no such thing as a Speenhamland Act; and whatever precisely may have been the results of this policy, it tended less to drive people off the land than to create an inert mass of pauperized rural labour. It is not true that in North America, population increase was " due initially to the greatly accelerated immigrations from Europe " (p. 823). North American population was already rising rapidly well before the great flood of immigrants arrived. Nor is it adequate to ascribe the increase in population in Britain after the mid-18th century simply to " a fall in the death rate as a result of greatly improved public health . . . the technology . . . of medical practice " (p. 823). The causation was far more complex than this, as some acquaintance with recent work in demography and economic history would show. The United States, we are told (p. 827), became " substantial exporters of pig iron to the United Kingdom from 1750 ". From the 1750's to the 1770's, British imports of pig iron from America averaged about 3,000 tons annually, as compared with some 45,000—50,000 tons per annum of bar-iron imports mainly from Sweden and Russia. And, as a final example, just what sort of an historical statement is this: " To this day the American workman likes machinery for its own sake, while the Englishman, with an atavistic but suppressed Luddism, is apt to fear its continuing advance " (p. 827) ?

There are, on the other hand, some pertinent and sensible observations about the vexed question of social conditions in the early nineteenth century; and it is good to see an appreciation of the need to think in terms of family incomes, and not simply of wage rates, when attempting to assess the gains and losses in living standards at that time. Some illuminating flashes light up the social scene: " The leisure pursuits of the upper classes were relatively untouched by the progress of technological development ".[1] They still are.

The chapter treads some ground already covered: by Ashby, in stressing the hostility towards scientific education in nineteenth century England; and by Ubbelohde in emphasizing the Platonic origin of the tradition which regards money and mechanical labour as vulgar and degrading.[2] To these it adds an element of its own:

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[1] *Ibid.*, p. 835.

[2] *Ibid.*, p. 821; cf. also pp. 779 ff. and Vol. IV, pp. 665–66.

the call to admire the business man slips in as Plato is deplored and the gentlemanly Establishment of education is humbled. " The public school system . . . produced . . . an educational climate unfavourable to industrial advance. The second and third generations in manufacturing families were not always able or willing to maintain the single-minded drive of their fathers and grandfathers, preferring to devote themselves to public life or to establishing a position in society. Energy which might have gone into new ventures was diverted into more socially acceptable channels ".[1] This may well be true. But what is to be the price of the assumption that new business ventures have a value which necessarily transcends the maintenance of certain social values or traditions? (And the public schools today show no sign of decaying through the absence of the sons of business men.) Across the Atlantic, things are different, in that New World where " the successful entrepreneur was an object of admiration rather than depreciation ", thus adding " then as now to the attraction of the United States for the emigrant ".[2] The educational Establishment in England has much that is bigoted in ideas and snobbish in practice. But what will be its counterpart in the new Technotopia, and how tolerant will it be? If education is increasingly " an essential ingredient in industry ",[3] will industry also become an essential ingredient in education? The path by which university education becomes a vocational training with its sole and gleaming goal the vision of a bigger bank balance may seem to have the merit of realism, but in reality it has the demerit of meaninglessness. The pervasive claims of " business + technology " to be the all-important sum of our day, to be at once the creator and guardian of an ethic automatically worthy of acceptance, are powerful and compelling. They beckon the Believer towards a Higher Standard of Living. *Quis custodiet custodes*?

The London School of Economics.

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[1] Vol. V, p. 821.
[2] *Ibid.*, p. 826.
[3] Ashby, *ibid.*, p. 776.

Book Reviews

An African Survey: Revised 1956. By LORD HAILEY. Oxford University Press for the Royal Institute of International Affairs. 1957. xxviii + 1676 pp. £5 5s. 0d.

The first edition of *An African Survey* was published in November, 1938, at the price of one guinea. It was reviewed in *Economica* in May, 1939, by which time a reprint had had to be issued. The project owed its inception to General Smuts, whose Rhodes Memorial Lecture at Oxford in 1929 had embodied a proposal for a survey covering all African territories south of the Sahara. The Carnegie Corporation of New York and, later, the Rhodes Trustees provided the finance, a committee with the late Lord Lothian as Chairman organised the work, and Lord Hailey undertook the Directorship of the Survey on his retirement from the position of Governor of the United Provinces in India. The material and drafts of sections and chapters were provided by a list of collaborators large enough to turn the Survey into an encyclopedia. That danger was averted by the remarkable contribution of Lord Hailey, who, with Miss Hilda Matheson as Organising Secretary, himself determined the scope and form of the survey, preserved the essential unity of a well-ordered and connected study and contributed opinions on trends and policy in the form mostly likely to commend themselves to the serious consideration of the governments responsible for the territories under survey, and of their advisers. The influence of the book on policy has been far-reaching. Moreover, in the British dependencies it was prompt. Thus the Report of the Rhodesia-Nyasaland Royal Commission (Cmd. 5949 of 1939) gratefully acknowledged the assistance which its members had derived from the Survey when preparing their recommendations. Again, in 1940, the White Paper issued by the U.K. Government (Cmd. 6175) explaining the intentions behind the Colonial Development and Welfare legislation, and announcing the allocation of a separate fund of government money for colonial research, said that " in reaching this decision we had in mind the proposal for special provision for research made by Lord Hailey in his *African Survey*, and we take this opportunity of acknowledging our debt to him for the suggestion ". For the next decade, Lord Hailey's report proved an indispensable guide and book of reference for administrators in the African territories. It provided an authoritative conspectus of the African setting, due account of which had to be taken in formulating policy within each territory.

After the war, conditions in Africa changed rapidly, and documentation became available on a vastly increased scale. The immense task of preparing a completely revised second edition of the *African Survey* was begun in September, 1952. Lord Hailey, then 80 years of age, decided that nothing less was required than a virtually new book, and he was not daunted. Once again, recourse was had to many specialists for the material on which the chapters were based, and half a dozen expert assistants undertook a large part of the burden of producing the basic drafts, while Lord Hailey himself accepted the responsibility for securing conformity throughout, both as regards the general scheme of the survey and the expression of views on the material presented. So far as possible the record of developments is complete up to the end of 1955. The new book has the great merit that it retains broadly the structure of the first survey, and nothing but the price prevents every addict of the old version from switching with immense advantage to the new.

This second great undertaking has, like its predecessor, quickly borne fruit. Lord Hailey's concluding chapter—the future of colonial research—appraises the achievement of research undertaken in the British dependencies under the provisions of the Colonial Development and Welfare legislation and parallel activity in the other African territories. He draws three broad conclusions. The first is that much of the most urgent and important scientific research must be broadly based and does not fit neatly into separate fragmented divisions: long or short term, pure or applied, natural or social sciences. The second is that the emergence of independent African states will probably result in a greater concentration of research activity, where locally initiated and financed, on immediate and urgent local problems within the capacity of local scientific personnel, rather than on longer term and more fundamental and wider problems. The third is that these wider problems are now recognised to have world-wide rather than mere colonial significance, requiring international as well as Commonwealth (rather than dependency) initiation, organisation and finance. He concludes: " In proportion as the subjects of research are thus recognised to be of more general concern, it is likely that those studies which are not of urgent practical significance may come increasingly to be regarded as no longer dependent on the support given to them by the Colonial Powers or the Administrations of Colonial territories. In that case the local institutions of research would tend to devote themselves mainly to the investigation of problems pressed for early inquiry or demanding a special type of experience for their solution ".

An African Survey: *Revised 1956* was published in October, 1957. In September, 1958, H.M. Government announced at the Commonwealth Trade and Economic Conference in Montreal that it had decided to establish an Overseas Research Council. The appointment of the Council followed in July, 1959. Its terms of reference provide that matters concerning scientific research in colonial territories, in Commonwealth countries and in countries outside the Commonwealth can equally be referred to it, and that in promoting research development it can look to possible collaboration between Commonwealth countries, countries outside the Commonwealth, international agencies and charitable foundations. Once again, Lord Hailey pointed the way forward. Africa is greatly indebted to this retired Indian administrator for his outstanding and devoted services.

ARNOLD PLANT.

Selected Papers on Economic Theory. By KNUT WICKSELL, edited with an Introduction by ERIK LINDAHL. George Allen & Unwin. 1958. 292 pp. 30s.

The Life of Knut Wicksell. By TORSTEN GÅRDLUND. Translated from the Swedish by NANCY ADLER. Stockholm Economic Studies, New Series, No. 2. Almqvist and Wiksell, Stockholm. 1958. 355 pp. Sw. Kr. 30.

With the publication of these two books, the presentation of Knut Wicksell to the English-speaking public is virtually complete. Before the war we had the *Lectures* and *Interest and Prices*, each containing also important supplementary articles. Since then we have had *Value, Capital and Rent*, and the inclusion in the International Economic Association's *Classics in the Theory of Public Finance* of Wicksell's central contribution to the theory of public finance. Now Professor Lindahl has gathered together the most important and hitherto untranslated papers on economic theory and Professor Gårdlund has written what must obviously be regarded as the definitive life. It is to be regretted that more of the *Finanzwissenschaftliche Untersuchungen* is not available. (There is a tiny fragment devoted to the taxation of monopoly in the American Economic Association *Readings in the Theory of Taxation*.) And some of us would very much like to have an opportunity of more detailed study of Wicksell's views on population theory which played so large a part in shaping his general social philosophy and his public activities. But for the history of economic analysis, what we have is certainly enough to establish him as one of the main figures in the formative period of modern economics. And Professor Gårdlund's *Life* extends the perspective by exhibiting him further in a role much less suspected in non-Scandinavian circles, that of an important social and moral reformer.

Apart from the authoritative editorial appraisal of Wicksell's general position as an economist, the main value of Professor Lindahl's collection lies not so much in any novelties which it contains as in its provision of background material for and elucidation of positions adopted elsewhere. There are indeed novelties, of which the reassessment of Cournot's theory of duopoly in the review of Bowley's *Mathematical Economics* and the contribution to the theory of tariffs in the last two papers of the collection may be singled out for special mention. But the chief interest lies rather in the papers in which, either by way of comment on the work of contemporaries, such as Pareto or Böhm-Bawerk, or by way of detailed discussion of particular analytical problems, such as the place of marginal productivity in the theory of distribution, the propositions of the final edition of the *Lectures* receive further explanation and illustration. Nevertheless, even taken by themselves, these papers would make the reputation of any economist: they contain the beginnings or the elaboration of some of Wicksell's most characteristic and important contributions, the influence of the rate of interest on prices, the solution of the " adding-up problem " in the theory of distribution, the restatement of the Böhm-Bawerkian theory of interest in terms of a marginal productivity analysis elaborated with regard to time, and the correction of the Walrasian doctrine of maximum satisfaction. It is a collection of which its publishers, who have contributed so much to the internationalization of our subject, may well be proud.

When we turn to Professor Gårdlund's *Life* we are likely to have some surprises. The reader of Wicksell's scientific work will have formed the impression of a spirit of exceptional candour and intellectual probity. But he will not have gained any impression of violence of disposition or impetuosity of judgment. It has been known, of course, that he was a strong neo-Malthusian; and the review of Cassel (reprinted in Volume II of the English edition of the *Lectures*) shows a powerful capacity for controversy. But many pure scientists have had very definite views on the population problem; and the gravamen of the critique of Cassel was just that, with his dogmatism, his ignoring of his predecessors and his quite preposterous claims to originality, he had offended so extensively against the code of scientific enquiry.

There is nothing here to disturb the impression created by the rest of Wicksell's scientific work where the calm unfolding of the argument and the studied moderation of the conclusions all go to suggest a spirit at peace with itself and its age.

In fact how different the reality. Professor Gårdlund's *Life* presents a personality much more like one of Ibsen's more apocalyptic characters than the popular idea of the pure scientist—a turbulent social reformer of marked eccentricity of personal behaviour and judgment, the quality and tone of whose scientific work is in sharp contrast with almost all the other circumstances of his life; a figure both nobly courageous and unbelievably naive, capable of risking his reputation and his livelihood either for the fine cause of a rational attitude towards the sexual problem or for a pedantic quirk of conscience which, when his belated appointment to a chair was in question, forbade him to use the traditional forms of address to the monarch in setting forth his application. Perhaps it is wrong to be surprised at all this. Most of the best natural scientists seem to have had the habits and dispositions of artists; and while there have been sedate and non-temperamental economists, they have seldom been very first class. Anyway, it is clear that from now on, those of us who have not known these things before will have to regard Knut Wicksell in a very different light. He was not only an important economist, he was also a leading influence in the general development of Northern European radicalism, one of the most significant social movements of modern times.

So far as economic policy was concerned, Wicksell's radicalism developed chiefly in two connections. In the sphere of production, he urged strongly limitation of population to be brought about not by Malthusian " moral restraint " but by recourse to deliberate contraceptive methods. In the sphere of distribution, he opposed the regressive tax system of his day, favoured mild progression in income tax and strongly supported the taxation of inheritance. He recommended what we should now call welfare services, particularly education. He also supported the principle of marginal cost pricing for public utilities. Politically he had much sympathy for the Social Democratic party, but he never became a member; and, while he sympathized in general with working-class movements and participated in many of their manifestations, he inclined to the view that socialism as prescription for increasing production emphasized the wrong points and ran some danger of diminishing freedom and total satisfaction.

In all this a certain parallelism with John Stuart Mill suggests itself. And this, of course, is no accident. After a profound religious crisis in late adolescence, Wicksell became a convinced Utilitarian and it is probable that he was considerably influenced by Mill's writings. None the less, there is a difference, marked on Wickell's side by a somewhat greater robustness in action and by a much greater proportion of straws in the hair. On the population problem, both held very strong, indeed, in some respects, perhaps, exaggerated, views. But although in his youth Mill had taken some part in birth control propaganda, in his maturity he was reticent, not to say mealy-mouthed, on this subject, whereas in the same connection Wicksell continually exposed himself to the full brunt of conservative and religious prejudice. On marriage, both held strong views on the undesirability of permanent ties unrelated to the welfare of children. But whereas Mill satisfied his scruples by a written declaration of no legal force repudiating his rights and then went forward to the normal form of civil marriage with Mrs. Taylor, Wicksell utterly refused any existing form of marriage, either religious or civil, and obliged the distinguished woman, Anna Bugge, who had decided to throw in her lot with him to rest content, despite her own wishes and those of her family, with a contract of his own devising which was in law no marriage at all. Again on freedom of speech both held the strongest views, and expressed them strongly. But it is difficult to imagine Mill capable of the gratuitous silliness of the burlesque account of the Virgin Birth which Wicksell thought it necessary to promulgate as a protest against the blasphemy laws; an " unforgivable stupidity ", to use his wife's words, which was responsible for his—equally unjustifiable—condemnation to a short term of imprisonment at a comparatively advanced age.

Perhaps Wicksell's greatest contribution to economic theory lay in the field of money and prices. Professor Gårlund's *Life* affords an interesting narrative of his various attempts to translate his theories into prescriptions for practical policy. In general he was a stable money man; he had no particular use for the gold standard; and during the First World War he was one of the most ardent defenders of the policy whereby the Swedish authorities repudiated the obligation to give notes for gold, holding that this gave a valuable opportunity for preventing further inflation. During the same period he was also a strong advocate of the policy of higher interest rates: and when, this policy not having been carried through, the price level had risen, he went so far as to urge measures which had the aim of reversing this process and restoring the price level of 1914. In this he was certainly ill-advised; the effects of the more moderate deflation which eventually came about are proof enough of that. But as a protest against inflation and the injustices thereof, the passage quoted by Professor Lindahl in which he defends himself against his critics still deserves to be taken seriously as does also a passage from an earlier article, published in 1898, in which he deals with the contention that a creeping inflation is good for the economy.

" Many people imagine," he writes, " that it would be possible to attain still better results if the value of money were, or could be, made not constant but successively variable downwards . . .They consider that the rise in commodity prices would be a continual incentive to industrial enterprise and thus have a fruitful and stimulating effect on the economic life of the community as a whole. It is not difficult, however, to realize that this is a pure illusion. What one person gains by rising prices another unfortunately loses: but it is a general psychological rule that an unexpected gain, other things being equal, can never compare in importance with an equally large unexpected loss. If, on the other hand, the rise in prices took place with such regularity that it could be foreseen and calculated in advance, why, then, it would of course be taken into account, in all transactions: nothing would be either gained or lost and we should only find ourselves after somewhat more trouble in the same position as if average commodity prices were constant. Were it therefore in our power to arrange these circumstances as we please, it is fairly certain that all interests would be best served if the standard of value were kept as scrupulously constant as the standard of weights and measures." Of course, *we* know the answer to all that. Or do we ?

It would be very unjust if the great interest of Professor Gårdlund's revelations regarding the details of Wicksell's life were to crowd out recognition of the merits of his book considered as a literary construction. As such it must stand high among the biographies of our profession: and the skill with which its author performs his difficult task deserves great praise. From the outset, with its description of the raw youth, with his religious crisis, his suppressed eroticism, his indolence and his bursts of energy, Wicksell becomes a real person; the middle years, with their intellectual triumphs and their political forays and frustrations, are set forth with clarity and candour against a fascinating background of the Swedish academic and social life of the period; and by the end we are left with a picture of almost Rembrandtesque intensity of the old *savant* with his private humility, his public aggressiveness, his acuteness and his credulity, with the square shoulders of a fighter and the brow of a major prophet, the blasphemer who knew by heart the hymn book and the psalter and the sermon on the Mount —a late nineteenth century " funny ", if you will, who was nevertheless one of the masters of our subject and, despite his occasional *bêtises*, one of the heroes of the struggle for a more rational social and moral order.

Professor Gårdlund notes " an undertone of irritation with regard to contemporary English economics " in several of Wicksell's last letters. In the summer of 1925, he wrote to Anna Bugge: " Yesterday evening and to-day I have been reading a long article on Marshall by Keynes, quite interesting but so absurdly English, as if no economists existed outside England ". Let us hope we have changed all that. But if the habit of mind complained of should still persist anywhere, Professors Gårdlund and Lindahl have certainly rendered it even less excusable.

ROBBINS.

The Structure of British Industry. A Symposium edited by DUNCAN BURN. The National Institute of Economic and Social Research. Cambridge University Press. 1958. Vol. I, xvii+403 pp. 45s. Vol. II, xii+499 pp. 50s.

The dust covers describe this survey as an up-to-date outline of the organisation and functioning of the principal British industries. No one who buys this symposium of nineteen industrial case studies and a final retrospective chapter should complain that he has not got his money's worth; and yet there will be few whose great satisfaction on reading them will not be qualified by a growing feeling of regret that the coverage was not more complete, a feeling which is not entirely mollified by reading in the editor's introduction how this came about. We are told that the chapters cover manufacturing industries responsible for nearly 40 per cent. of the net income of all manufactures, and only approximately one-third of employment in them (1955). One does not need to have spent one's working life in, for example, the heavy electrical industry, or in food, confectionery, tobacco, drink and feedstuffs, or in furniture and floor coverings, or non-ferrous metals, or apparel and boots and shoes, rubber products, scientific instruments, computers, photography and horology, or printing and publishing, to feel disappointed that this or that important industry has not made the grade of principal in an enterprise which deserves to hold the limelight among realistic industrial literature for some years. Still, what's in a name ? A symposium nowadays can mean anything from a drinking party to a collection of essays by different writers upon a given subject, and every reader of these essays will hope that the authors were provided with the ample refreshment they so richly earned. Here and there indeed are signs that they needed it even more than the reader.

While several important British industries are omitted, two fields of enterprise are included which fall outside the normal industrial classification, namely agricultural production and marketing, and inland carriage by road and rail. Any doubts about the appropriateness of including agriculture, and of assigning to it the opening chapter, are immediately dispelled on examining the main themes which Professor John R. Raeburn has selected for discussion, the procedure he has adopted in marshalling and analysing the data, and the cogency of the conclusions which he draws. He concerns himself with the structure of this British industry, as affected by overseas competition in the home market and by State intervention in its factor and product markets and also in internal management. He passes on to consider the structure of marketing agricultural produce and the effects of the development of forward pricing arrangements with State guarantees, and of Marketing Boards with monopoly powers. Finally, he assesses the economic efficiency of the industry in both production and marketing, basing his judgment on an admirably chosen set of seven general criteria (set out on p. 21), with an added injunction concerning the importance of minimising real costs incurred in meeting the special requirements which the State may impose upon the industry. His conclusions are that structure and efficiency are broadly right for the attainment of the farmers' own purposes, but not so efficient for achieving, without state aid, the government's special requirements; and that the methods adopted by the State to get what it wants have involved or will involve longer-run economic consequences and difficulties that have been largely ignored. Professor Raeburn has provided a model survey, both in choice of method and in power of execution, leading inexorably to conclusions which rank as authoritative judgments rather than mere expert opinions.

Professor Raeburn brought to his task a long specialised experience in the field. So also did Professor Gilbert Walker and Mr. Christopher I. Savage, who provide in their chapter on Inland Carriage by Road and Rail an account of post-war legislation and developments. As regards road freight vehicles, statistics quoted show that in 1955 there were over 1,100,000 licensed to nearly 550,000 holders, of which 940,000 vehicles were run by 470,000 private traders, operating under ' C ' licences which debar the traders from carrying any goods but their own. Before 1933 the private trader was free, in particular, to contract, as occasion offered, to carry back-loads, and commercial clearing houses existed in many traffic-loading centres to arrange the service at acceptable rates. The figures show how great a convenience it still is to industrialists and traders to run their own vans and lorries, and it is surely worth consideration how much smaller their contribution to road congestion might be if the right were restored to them, as it was temporarily during the Suez petrol crisis, of sharing the space in their vehicles on mutually satisfactory terms. The authors estimate, on the basis of a road traffic sample carried out in September, 1952, that " British Railways, the public carrier by road, whether publicly or privately owned, and the private carrier using a ' C ' licence appear to be sharing about equally between them the total traffic in general merchandise. Each is carrying a round 50 million tons, the railways on the average for the longer distance—126 miles by rail against 85 miles by road ". Attention is called to the economic problem with which licensing authorities have been confronted by the new power conferred on them under Clause 44 of the Road Traffic Act, 1956, to suspend or revoke an ' A ' or ' B ' licence should the operator consistently charge for carriage by road " sums insufficient to meet the costs of rendering those services ". " Clause 44, if the administrators should prefer . . . the full allocated cost of the accountant to the marginal cost of the economist, might limit, only too readily, the opportunities now open to carriers by road to enlarge their businesses by reducing their rates ".

Turning now to the essays on British industries as more usually understood, the editor's introduction explains why recourse was made to contributors some of whom were already authorities on particular industries, others who had some familiarity with the kind of problems involved, and others again who were prepared to break what for them, if not for us all, was new ground. The authors were provided with no general pattern or specification to which they should conform, and each was responsible for deciding how selective he would be and where emphasis would concentrate. This procedure has its obvious disadvantages. The titles of some of the individual chapters are laid open to the same criticism as can be levelled at that of the book. They seem to promise more than they give. On the other hand, it was obviously right to give the expert contributor full latitude to define his subject and treat it in his own way, and if no more accurate short title could be found to leave him to explain at the outset the limits he set himself. Where this has been done, much has been gained by providing adequate treatment of the areas selected for examination, and in particular cases the selection made has served to emphasise the diversity of problems confronting different product divisions of the so-called industry.

Thus Mr. W. B. Reddaway, who contributes Chapter VI on The Chemical Industry, opens his account with an explanation of the problems of definition, the reasons for diversification of output by individual firms, their size-distribution, the nature and extent of competition between them and the special problems in the industry arising from the importance of uncertainty and risk. He then selects four commodity groups as case studies—sulphuric acid, alkali, dyestuffs and plastics—and examines them in depth. He has important suggestions to offer for mutually advantageous changes in protective tariff policy between the various countries producing dyestuffs, in the interests of greater efficiency in production all round (p. 252), and for an improvement in the British system of protection under the Dyestuffs Act, which still offers a would-be importer no intermediate stage between an import licence duty-free and complete prohibition.

The Aircraft Industry, which is the subject dealt with by Professor Eli Devons in Chapter XI, owes its technical progress, its development and its structure in the main to the demands of the fighting services, compared with which the civil market has been small. The main contractors are relatively few, and by no means all specialised on aircraft orders: the firms supplying the important materials, equipment and components are many, and very few of them specialise on aircraft work. Here again, the pattern of company structure and the diversification of output at all stages reflects the risks and uncertainties inherent in the erratic fluctuations of the type and volume of governmental demands for aircraft, and Professor Devons has rendered a valuable service by concentrating on the difficult problems facing the main aircraft and aero-engine firms, while warning the reader of the need for a parallel study of sub-contractors and of the arrangements for co-ordinating the production flows throughout the industry as a whole. His highly informative account of the relations between military and civil demand, and between governmental and private finance of prototype development with recoupment of government subsidy from royalty payments on sales of civil aircraft, is indispensable reading for all who aim at understanding the basic issues underlying the rapid and drastic changes at present occurring in the industry.

Three contributors in particular had the advantage of approaching their assignments as established authorities. Professor A. Beacham's comprehensive study of the Coal Industry (Chapter IV) is sympathetic to the social problems which the National Coal Board has decided to carry on its own shoulders, but none-the-less critical of the basic principles of coal pricing by which the authority has endeavoured to recoup the consequential high costs from coal users. He challenges the idea " that prices should reflect the average cost of producing and transporting coal to the consumer, that relative prices should reflect ' quality ' and that coal of the same grade in the same market should sell at the same price, irrespective of the pit of origin or the type of consumer ". A valuable discussion leads to the conclusion that average-cost pricing should go, and that areas should be allowed to determine their own price policies, competing for business on the basis of doing the best they can for themselves. Professor D. C. Hague contributes Chapter XVI on the Man-made Fibres industry, and in the course of his account of the development of polyester fibre (Terylene), acrylic fibre (Orlon), protein fibre (Ardil) and later variants, he demonstrates " how much competition there is in a basically monopolistic situation ". Mr. H. Townsend also is on familiar ground when providing his study of the Cutlery Trade (Chapter XIX) and he succeeds admirably in introducing a light touch which makes his instructive account easy and pleasant reading.

Limitations of space alone prevent proper notice of the essays contributed by authorities with special knowledge and experience. Professor A. K. Cairncross and Mr. J. R. Parkinson write on the Shipbuilding Industry, Professor C. F. Carter on Building, Mr. G. W. Furness (formerly of the Cotton Board) on the Cotton and Rayon Textile Industry, Mr. G. F. Rainnie on the Woollen and Worsted Industry, Professor B. R. Williams on both the Pottery Industry and the Building Materials Industry, Mr. C. J. Thomas on the Pharmaceutical Industry.

Mr. A. Silberston surveys the Motor Industry and presents a well-balanced discussion of the familiar criticisms directed at the leading manufacturing firms, that they offer too wide a range of vehicles, in too many price ranges, with too frequent model changes. He examines the relative bargaining strength of component manufacturers and vehicle makers and concludes that they are now fairly matched ; and he weighs judicially the pros and cons of greater or less reliance on sub-contractors. Professor T. Wilson grapples vigorously and entertainingly with the diverse collection of products which incorporate electronic devices, i.e. which involve the use of valves or transistors, and which are made by firms that might for certain purposes be called the Electronic Industry. Because radio engineers and physicists are largely mobile between widely different product manufacturers, he sees an advantage in accepting a wide definition. In his account, however, he concentrates mainly on the production and distribution of radio, radar and television equipment, and has much to say about the relative merits of full-cost and marginal-cost pricing, and on price maintenance and after-sales service, which will set undergraduate students thinking. On price flexibility they are likely to be bewildered by his suggestion (p. 171) that barrow-boys exploiting a temporary shortage of bananas raise their prices by restricting output.

I have an especially warm feeling of gratitude, which I am sure will be widely shared by other readers, to Messrs. M. E. Beesley and G. W. Troup for their chapter on the Machine Tool Industry, and particularly for the account which they provide of its marketing structure. Distribution of British machine tools is largely organised through a system of sole agencies, which secure exclusive rights to sell and service individual types of machine tools within a manufacturer's range. The merits and demerits of this arrangement, in contrast with the American system of selling, are well argued. In order to assist new entrants into the industry, the authors incline to the view that so far as the home market is concerned it would be advantageous if the exclusive agency were made legally unenforceable.

There remains for notice the three important chapters written by the editor himself. Mr. Duncan Burn has brought his vast knowledge, unrivalled among detached students, to bear on his account of the Steel Industry, and provides a succinct description of its structure, its technological developments and its post-war expansion. He describes the progress made by the Realisation Agency in selling firms back to private ownership following the denationalisation Act of 1953. He explains the powers and responsibilities of the Iron and Steel Board, thereby established, to supervise the industry ' in the public interest '. It may fix maximum prices; it operates the Industry Fund, fixing the scrap levy which is intended to equate the cost of using pig-iron or scrap in steel furnaces, and spreading the loss incurred by supplying producers with imported ore or other steel-making material at prices below those actually paid for the imports by the central buying organisation. It therefore has wide discretionary powers, which may be exercised in the pursuit of debatable policies or, because of the uncertainties of the market, may result in serious losses. In a cautiously worded conclusion, after glances at the United States industry, which operates without any such supervising body, and at trends in the European Coal and Steel Community, he implies that the Iron and Steel Board is very much on trial and that the steel producers might serve the public equally well without it.

Mr. Burn contributes secondly a chapter on the Petroleum Oil Industry. The oil industry in Britain perhaps cannot be properly understood except as part of the international structure of the industry with all its complicated pattern of concentration and vertically integrated groups. The international structure is first examined, then the part played by British companies abroad and by firms in the United Kingdom, and finally price formation and competition in the international setting. I will not venture to summarise further this very gallant attempt at summary treatment of a vast topic, but I feel bound to question whether the decision to include a survey planned in this way in a book intended for the non-specialist reader was really well taken.

The final chapter " Retrospect " is also contributed by the editor. It " draws together and discusses what is said in the book on some matters of common interest "; e.g. standards and criteria for assessing productivity and performance, competition and incentives to efficiency, the influences on structure of problems of finance and the impact of taxation, and the impact of the State on the use of resources. Mr. Burn raises many questions in a provocative form, which academic readers may be tempted to paraphrase for the torment of unfortunate examination candidates. I must not attempt to debate them here.

The production of this important survey was financed by funds under the Conditional Aid programme. The editor and the contributors are to be congratulated on their product.

ARNOLD PLANT.

A New Prospect of Economics. By MEMBERS OF STAFF OF THE DEPARTMENT OF ECONOMICS IN THE UNIVERSITY OF LIVERPOOL. Liverpool University Press. 1958. xiv + 498 pp. 30s.

Price Theory. By W. J. L. RYAN. Macmillan & Co. Ltd. 1958. x + 396 pp. 31s. 6d.

The books reviewed here raise quite fundamental problems in the teaching of theory in economics. An attempt at their appraisal must, therefore, be preceded by a few words on these wider issues.

Speaking very broadly economic theory has two aspects, the scientific or philosophical and the didactic. In its scientific aspect it purports to explain certain forms of our behaviour in terms of general hypotheses. In physical sciences theories of this kind enable us to make predictions of events which have not been observed so far (sun eclipses or atomic explosions). But in economics the predictive power of the existing theories is limited to events of a very general character, covered by such well-known generalisations as the law of demand or the upward-sloping form of the supply curve. From a strictly practical point of view their value is negligible. It is therefore only at advanced stages of university economics that the scientific and philosophical aspect of economic theory becomes relevant.

In its didactic aspect economic theory takes the form of models devised for no other purpose than to give a general idea of how the economic system works. They are like simplified drawings of a machine or of an organism, which help us to see the general principles of their construction and the interdependence of their component parts. They are useful because, if properly presented, they bring out the basic interrelationships within the economy. But the resemblance of these models to the real world is not such as would permit us to derive from them, by calculation or by simple inference, any predictions of actual events.

For a theory to have a didactic value a bridge must, however, be built between the model and the reality the model is expected to elucidate. This is what we usually understand by application. The relevance of economic models to any real phenomena is by no means obvious, and unless the models are used as a basis for a more general discussion in which the other variables (those abstracted from in the model) are also taken into account, a dichotomy between theory and reality is likely to appear, and the didactic value of the theory is lost. The more abstract and complicated is the model, the more difficult is the task of building this bridge. It seems, therefore, that at least in the initial stages of teaching economics there is some complementarity between theory and non-theory teaching. The question is not how much of one in preference to the other, but how much or how little of both of them.

How do the two books under review fit into this scheme? Let us take first the *New Prospect of Economics*. It is called an introductory textbook and is expected to be used by undergraduates in the first two years of their university course. It covers the wide field of the theory of value and distribution, of money, income determination and public finance. The stress is on theory. With the exception of the chapters on social accounting, government finance and the organisation of international trade, the book does not give any factual information; and where an attempt is made to build a bridge between the abstract models and the real world, it usually takes the form of simple generalities and qualifications.

The choice of topics is not always in line with the introductory character of the book. The theory of games, for instance, or some advanced parts of welfare economics, indifference curve analysis and utility theory, are not likely to be properly understood by a beginner. More elementary points, on the other hand, are often treated very summarily or only hinted at. Very little, for instance, has been said about the distinction between shifts of a demand curve and movements along it. In the discussion of demand for the factors of production no account has been taken of the possibility of substituting factors one for another. Full-cost pricing has not even been mentioned. The theory of international trade has been dealt with very briefly indeed; the effects of devaluation have been discussed without even the slightest reference to elasticities of demand and supply. On the whole the book thus makes the impression of being a compromise between the more esoteric interests of some of its authors and their intentions to write a down-to-earth introductory text.

The book has been written by a large team of specialists and in consequence the treatment is very uneven. Some of the chapters, specially those contributed by Mr. Clayton (on income flows and money) are very readable and may be of great help even on a very elementary level. (One may wonder only why the word " enterpriser " has been substituted for the already domesticated " entrepreneur ". The reviewer is inclined to agree with Edwin Cannan that it sounds a little too dashing.) The chapter dealing with statistical sources, written by Mr. Osborn, contains much information which may be useful in any further research. Some of the other chapters, however, specially those in the field of the theory of value, seem to be overloaded with geometry and do not always make easy reading. There are also some lapses in exposition, which the reader may easily find misleading. The argument, for instance, that if the variable factor is measured in sufficiently large units, its marginal product must decline straightaway (p. 89) suggests that the form of the marginal product curve depends on the choice of the unit of the variable factor—which is not true. The discussion of devaluation (p. 385) has a trace of circular reasoning: devaluation affects demand for and supply of foreign currency, which in turn affect the exchange rate. Also the headings of some chapters and sections do not quite correspond to their content. For instance, the chapter entitled " Time in Economics " deals nearly exclusively with the marginal efficiency of capital, and the chapter-group dealing with the basic concepts and relationships of investment, consumption and saving is called " Theory of Economic Growth ".

Dr. Ryan's *Price Theory* is much less ambitious in scope; the field it covers is strictly that of the theory of prices. And it is intended for students who wish to take economics as their special subject at university. Much effort is, therefore, put into spelling out all the assumptions and making the reasoning rigorous. The theory it expounds is orthodox. The author himself describes its main parts as a restatement of the Marshallian theory, and is aware of the possibility that it may soon become obsolete. Its exposition follows the well established pattern of (1) the household, (2) the firm, and (3) pricing of goods and factors. There is hardly any controversial material in it. Although limited in scope the book has thus the character of a textbook, the main stress being laid not on originality but on accuracy of thought and clarity of exposition.

From the technical point of view the book is undoubtedly a great success. The exposition can hardly be improved upon. The reader is led very gracefully over the successive steps of even the most intricate parts of the theory and is taken into confidence on delicate matters of its purpose and general methodology. Extensive use is made of geometry, the argument taking, too often perhaps, the form of deriving one diagram from another. The philosophy underlying this procedure is that if we are properly drilled in this diagrammatic economics, we may be able to think in diagrams about actual events, and this will make our thinking more intelligent and precise. This philosophy may not be accepted by everybody, but even those who reject it must appreciate the craftsmanship of the performance.

Minor blemishes, which might attract the attention of a pedantic reader, can be easily taken care of in the second edition. Thus, the nature of the indifference curve could be made a little more precise. At present it does not seem to be clearly defined either operationally or introspectively. The section on a multi-product firm could be expanded and flexibility requirements taken into account. More precision could be achieved in the discussion of the long-run supply curve of labour, which sometimes is interpreted as the supply curve of workers and at others as the supply curve of the services of labour. But on the whole, if Dr. Ryan's book can be criticised, it is not on technical grounds. Rather the opposite is the case. There is a danger that just because it is so readable and lucid, an inexperienced student may not appreciate properly the advanced character of its subject, and may take it for just another textbook to be read through as part of a general course. In fact, if we compare the two books under review, one recommended as an introductory textbook and the other as one for specialists, there is not much difference between them either in the scope or in the method of treatment of their common field. Both subscribe to the same philosophy of diagrammatic marginalism and, although there may be differences in the degree of awareness of the objective pursued, both tend to represent the economy as a system of smooth and continuous interrelationships, very little perturbed by institutional factors and unanalysable frictions.

This formalism and love of abstraction can now be found in many textbooks intended for both specialists and general students. The books reviewed here are but two distinguished examples of what may be regarded as a general tendency. Are we, however, certain that this is a right tendency? Taken individually, many of the textbooks published recently may be quite remarkable each in its own field; but is it not possible that in the aggregate, because of a uniform bias in the choice of that field, they may give a distorted view of our discipline ? What a general student needs is some knowledge and understanding of the world around him in order to be able to deal successfully with the practical problems with which he may be confronted. In principle, thinking in graphs and formal interrelationships may be an admirable method of reaching this end. In practice, however, the scope of studies being limited by the time-table, the programme may not be feasible because of lack of time. And the actual result may be not knowledge and understanding but merely bewilderment and a sense of frustration.

These concluding remarks must not be understood as a criticism of any particular book. They rather make up a plea for a more balanced aggregate output, in which due regard would be paid not only to the provision of tools but also to the material in which we work.

S. A. Ozga.

Games and Decisions. By R. Duncan Luce and Howard Raiffa. John Wiley and Sons Inc., New York. Chapman and Hall Ltd. 1957. xix + 509 pp. 70s.

Economists vary in their views of the theory of games from outright rejection to doubts about its value so far, to complete acceptance of it as the proper foundation for micro-theory. The attitude of any economist will depend on his tastes, his judgment, and the state of his knowledge of the subject. Influencing him, therefore, requires both works of propaganda and works of communication. Luce and Raiffa consider their book to be of the latter variety; but to the extent that they frequently argue that what they explain in it also makes sense, they fulfil in addition the former function.

They have many objectives. One is to explain without the use of advanced mathematics the current state of the theory of games. By this is meant not merely a description of the extensive form of games and a detailed examination of the two-person zero-sum game with a finite number of strategies, but also an explanation of non-zero sum games, co-operative games, and various extensions and alternatives to the von Neumann and Morgenstern theory of n-person games. Thus, they devote much space to the work of Nash, for example, on the concept of a solution for a non-co-operative game, and the study of a two-person bargaining situation, to that of Shapley on the value of an n-person game, and to that of Kuhn, Tucker, and many others who are in the forefront of current research. The significance of this work for economists lies in its relevance to the theory of market behaviour. For those who have tended to equate game theory with the zero-sum two-person problem, chapters 5, 6, 7, 9 and 12 should be eye-openers. One question of great importance which is discussed there is whether it is always possible for ostensibly competitive firms to collude implicitly and reach the monopolistic optimum by exchanging information and guarantees, so to speak, via actual market behaviour. (I might add at this point that I would not recommend this book as a first introduction to game theory. Some combination of Williams, Mckinsey, Kuhn's lectures, and selected parts of chapters I, II, III and IV of von Neumann and Morgenstern would be preferable.)

A second objective is to discuss the application of game theory to social phenomena. In fact, Luce and Raiffa deal with little more than the analysis of the operations of a simplified voting body, and the workings of a simplified market with one seller and two buyers. They then refer the reader for additional economic application to Martin Shubik's brilliant new book, *Strategy and Market Structure*. This shyness on their part is most unfortunate, because its chief result is to convey the impression (entirely false) that game theory in economics has made little or no progress since 1944 when the three-person market was first investigated by von Neumann and Morgenstern. Indeed, on the question of applicability Luce and Raiffa seem to be pessimistic. In my opinion this arises from their adoption of a quite unsound methodological position. Thus they refer to the descriptive inadequacy of existing game theory, the fact that its assumptions about knowledge, perception, memory, calculating power, and utility rarely, if ever, hold true in the real world. Their emphasis on descriptive power rather than explanatory power as a desirable characteristic of a theory is certainly opposed to modern methodological writings (notably Karl Popper's), and the reader would be well advised as a counterweight not only to read Shubik's book but also to become acquainted with the various papers by Oskar Morgenstern on the subject. The only tests (other than being complete, lacking self-contradictions, and refraining from the use of *ad hoc* arguments) which game theory has to meet are in terms of its relevance to the phenomena which it seeks to explain. While an empirical investigation of assumptions might be one way to this end, the danger of relying entirely on this approach in testing abstract theory is now widely recognised, and a more satisfactory procedure is to test the positive hypotheses which may be obtained lower down the chains of derivation of the theory.

At this point another methodological confusion must be referred to. Luce and Raiffa say that game theory does not state what a player should do. By this they appear to mean that it does not prescribe his ends. It is, however, conditionally normative in the sense that it tells him how to achieve his ends. Moreover, in one place at least they say that game theory is not descriptive in the sense that it describes how people do behave. It is not clear how a theory which does not or could not explain actual behaviour can be relevant to potential behaviour. It is also difficult to see why a theorem such as the minimax theorem should be only normative when the proposition that marginal rates of substitution equal relative prices at the point of maximum utility is not usually interpreted in that way.

Two other topics which the book discusses deserve special mention. These are decision-making under uncertainty and group decision-making, and Luce and Raiffa provide first-class introductions to both of them. Their treatment of Arnow's theorem is specially to be recommended both for its simplicity and clarity.

The book under review is a long one. Apart from those sections which have already been discussed, it has eight technical appendices on such matters as utility and probability, the minimax theorem, and linear programming. In addition, the text itself goes into a large number of subjects not mentioned in this review. It contains, for example, many stimulating comments on utility theory. It would be a pity, therefore, if it did not reach the widest possible audience, not only of economists but of social scientists in general. Although, like the reviewer, they may disagree with much of the contents on methodological grounds, they cannot fail to be impressed by the erudition with which they will be confronted. Furthermore, while they may not be converted by what they read, the widening of horizons, both technical and conceptual, following on an appreciation of game theory, should be of great value in pursuing more traditional approaches to the behavioural sciences.

M. H. PESTON.

Foreign Trade and Finance. Edited by WILLIAM R. ALLEN and CLARK LEE ALLEN. The Macmillan Company. 1959. xii, 500 pp. 20s.

Teachers will find it convenient to have, collected together, this selection of outstanding contributions published during the period 1943 to 1958 in the field of foreign trade. Most of the articles included in this publication were written after *Readings in the Theory of International Trade* was published by the American Economic Association. The present book includes such well known articles as Lloyd A. Metzler's " Tariffs, the Terms of Trade and the Distribution of National Income ", F. Machlup's " Three Concepts of the Balance of Payments and the So-called Dollar Shortage ", G. D. A. MacDougall's " A Lecture on the Dollar Problem ", and Ragner Frisch's " On the Need for Forecasting a Multilateral Balance of Payments ".

Another welcome article is Professor Haberler's useful contribution on the practice and theory of convertibility (" Currency Convertibility ", American Economic Association, 1954). In the reviewer's opinion this article does suffer, from the point of view of teaching, from the emphasis which entrenched Free Traders place upon the desirability of international trade. We say " suffers " because the exposition might mislead students not acquainted with the history of controversy in this field. Professor Haberler rejects (page 454) the assertion that higher productivity in the United States than elsewhere will threaten other countries with being undersold in all markets, with the statement: " This . . . is bad economics because trade and the profitability of trade to all concerned depend on *comparative*, not *absolute* cost . . . Only if America's advance is more rapid in import-competing industries . . . will America's trading partners be hurt. This does not mean that trade is no longer beneficial. It only means that it may be less beneficial than it was before . . . ". This is far from clear. Will not students be tempted to reason as though when a country's production improves in such a way as to leave her comparative costs the same as those abroad, she suffers economic loss owing to the cessation of trade? The exposition would be clearer if more stress were laid upon the desirability of high production and consumption, and less upon the desirability of a large volume of international exchange. If international exchange is desirable in itself, this is for political or social reasons rather than economic ones.

It is appropriate that the collection should include Mr. Hansson's article from the *American Economic Review* of 1952, on " A General Theory of the System of Multilateral Trade ". To-day, when students are struggling on the theoretical side with the wealth of recent analysis of the rôle of factor-endowments in governing international trade, this article of Mr. Hansson's constitutes an attempt to give some representation of the empirical facts about the differing factor endowments of various regions in the world. Any such simplification must of necessity be an over-simplification. Mr. Hansson considers only three factors of production—labour, land and capital—and only three measures of quantity—ample, moderate and scarce.

Clearly, such a representation of the facts can be schematic only, but the over-simplification has, of course, the virtue of reducing the highly complex picture to certain broad features. Particularly interesting in his discussion is the study of the way in which different areas change their factor-endowments. Thus Mr. Hansson points up the United States' development from a country with much land, moderate labour supply and little capital, through the stage of much land, moderate supply of capital and little labour, to the stage of much capital, moderate supply of land and little labour. This development will probably go still further in the United States. It is to be expected that through the growing population and the rising living standards, the demand for food will steadily rise; together with this, growing industry will mean rising demand for the raw materials of manufacture. Thus, the pressure on the land will become more marked and it is to be supposed that the United States, like Europe, will be characterised, not only by abundant capital, but also by scarce land. As this materialises, the world trade pattern will be much affected, the United States being in more direct competition, both in respect to her exports and in respect to her imports, with Britain and the continent of Europe.

H. MAKOWER.

A Time Series Analysis of Interindustry Demands. By K. J. ARROW and M. HOFFENBURG with assistance of H. MARKOWITZ and R. SHEPHERD. Contributions to Economic Analysis, 17. North-Holland Publishing Co., Amsterdam. 1959. 292 pp. 55s.

This book summarizes work by a number of American economists and statisticians, over a period of six years, on inter-industry demands. The object was initially to reveal the structure of demands by each industry for the products of all industries—that is, the flow of goods between industries. Although the authors emphasize the role of inter-industry demands in determining general equilibrium they do not over-estimate the importance of their chosen subject. As they soberly remark, " it is not contended that the determinants of inter-industry demands are more important to an understanding of economic problems than the determinants of final demands or of the demand for labour, but the latter . . . do not suffice to make predictions " (pp. 2–3).

The very careful work, both in the theoretical model-building and in the preparation of the statistical material to which the model is applied, yielded some conclusions of considerable general interest. Chief of these is the conclusion that the input-output ratios of American industry varied substantially over the period 1929–52, so that Leontieff's original " fixed coefficient " assumption is not valid. Further, for the period under review it was found that three factors exerted an important influence upon the input-output ratios, namely, changes in the pattern of output of each industry (during this period the great shifts from peace production to war production and back violently affected the output pattern of most industries); changes in techniques of production and consumers' tastes; and changes in the smoothness of running any given production job. In the authors' opinion their research work " points to the existence of some model which will give a theoretical explanation of changes in input-output ratios ", but the authors regard their own model as inadequate (p. 132). The experience of the writers with this research causes them to emphasize the limitations of time-series analysis of economic data. " The paucity of independent variation in the data and the unavailability of many historical series together constitute a formidable obstacle to any disaggregative elaboration of economy-wide econometric models to be fitted to time series " (p. 133).

The conclusions arrived at do indeed depend very largely on the selection and rejection, at every turn, of relationships and figures. The full benefit of the study must accrue, not to those who read it, but to those who wrote it. " The fitting together of diverse statistical series for analysis is something of an art rather than a science. Other analysts might have selected different basic series and different procedures, with perhaps different results " (p. 98). The book is to be strongly recommended to anyone thinking lightly of embarking upon econometric work. While being highly suggestive, it cannot fail to warn any serious student that there are difficulties at all levels in this kind of work. The fundamental difficulties of submitting economic hypotheses to empirical tests are discussed in chapters II and III, which should help to prevent further additions to the already large volume of meaningless " econometric " work. But the issues briefly treated here are very far-reaching; and for the beginner it may be more useful to work himself into the subject from the other end, that is by examining the way in which the statistical series used have been arrived at. Mr. Russell T. Nichols, who is responsible for the statistical appendices as well as sections of the main manuscript, has presented the results, methods and sources with admirable completeness and clarity. The high quality of this contribution is clear to anyone who has done work of this kind.

H. MAKOWER.

Portfolio Selection. By HARRY M. MARKOWITZ. John Wiley and Sons, New York. Chapman and Hall Ltd. 1959. x + 344 pp. 60s.

A potential reader of this text, attracted by the title (*Portfolio Selection*) or by the sub-title (*Efficient Diversification of Investments*), would do well to notice also that it is No. 16 in the series of monographs published by the Cowles Foundation. The book nowhere gives simple guidance to investors on how best to diversify their holdings. Indeed, the point is made that there is no half-way house between the private " hunch " and full-scale mathematical programming. When more than a few securities are considered, the programming drill needs to be organised for electronic computation. This monograph takes its proper place alongside such earlier econometric work of the Cowles Foundation as Koopmans' *Activity Analysis of Production and Allocation.*

Having said this I would quickly add that Dr. Markowitz's application of modern econometric techniques in this new field is not of the fearsome mathematical content so often found in the work of the Cowles Foundation. He has written his monograph with the non-mathematician primarily in mind and he has in fact achieved a clarity of exposition which is quite remarkable.

On the other hand, he has not succeeded completely in producing a well-balanced text to be read straight through by a non-mathematician. I would not regard this as feasible in a mathematical-programming application of this kind and at the present pioneering stage in its development.

What the reader needs is not only the basic ideas of probability and statistics, which many non-mathematicians have acquired; to get far into the subject he must have been brought up on the concepts and techniques of matrix algebra. This is something we cannot yet assume, at least in this country. I would not agree altogether with Dr. Markowitz's comment that the principles of matrix algebra " are not difficult to learn, even for the reader with a meager mathematical background " (p. 154).

The difficulty is clear from the arrangement of the text. The central section, chapters 7, 8 and 9, deals with the mathematical-programming problem of selecting " efficient " portfolios of securities. The definition of efficiency adopted has to do both with the average or expected return and with the variability of return. The objective is to increase expected return for given variability, or to decrease variability for a given expected return. An efficient portfolio is such that its variability cannot be decreased without also decreasing expected return, and that its expected return cannot be increased without also increasing variability. This section can be read with great profit even by those experienced in mathematical programming, particularly since it involves " quadratic " and not just " linear " programming.

However, the author tries to make this central piece of work accessible to the non-mathematical economist and to relate it to general economic ideas. The first six chapters deal with the objectives of portfolio analysis and with the main concepts of probability and statistics relevant to the analysis. These chapters read very well indeed, but I would maintain that they are not needed by mathematical programmers nor are they enough for those without experience in this field.

Then, after the main section, there is a further development in chapters 10 to 13 concerned with the relevance of rational choice under uncertainty to the specific problem of portfolio selection. Again this is excellent as an introduction to the present highly fashionable utility theory of choice. Dr. Markowitz has given, however, no more than some general ideas on the application of such utility theory to portfolio analysis. He leaves us rather uncertain on the practical relevance of what he says to his more definite development of mathematical programming of portfolio selection.

The book, then, has great merits as an exposition and it marks a considerable step forward towards the solution of the practical problem of portfolio selection. It is limited, perhaps inevitably, to one specific aspect of " efficiency " and it ignores such diverse problems as the timing of sales and purchases of securities which any investor must have to consider. It would not convince an institutional investment manager that he must immediately go out and recruit a mathematical programmer. But perhaps it is pointing clearly in that direction.

R. G. D. Allen.

The Wage Rate Under Collective Bargaining. By J. Pen, translated by T. S. Preston. Harvard University Press. Oxford University Press. 1959. xiv + 216 pp. 48s.

The author is Professor of Economics at Groningen. His object is to provide a theory of the processes by which a money wage actually gets set in bargaining: he is not concerned with the reactions to the wage once set, except in so far as they enter into the expectations of the bargainers. His account begins with a contract zone bounded by the wage rates at which one or other of the parties will break off dealings altogether. Within these limits the two parties are dependent on each other. A measure of this dependence can be conceived for each party. When a given wage is proposed, a party's spokesman will set a certain valuation upon it as a working arrangement, and he will also set a certain valuation upon the conflict which may ensue if he does not settle for it: the excess of the first valuation over the second is the measure of his dependence, and his own economic power varies inversely with this and directly with his ability to heighten his opponent's sense of dependence. The higher the wage proposed, the greater the economic power of the employers, the less that of the union, and the process of bargaining consists in finding the wage at which the two powers are equal. This process may be analysed by noting that at each stage of it a spokesman has a choice between settling for certain terms forthwith or holding out for better, and if he does hold out, he may get the better terms or he may land himself in a conflict. The valuation he sets on each of these two outcomes must be weighted by that outcome's probability. The sum of the two weighted valuations gives us the composite valuation he sets on holding out, and he will be ready to settle when this is no higher than his valuation of the terms of settlement currently open to him. But also, men are apt to like or dislike risk-taking for its own sake, and a spokesman who likes it will set a higher valuation on holding out than the actuarial probabilities alone would warrant, and conversely. The tactics of bargaining consist in working on one's opponent's valuations so that he will come to see less advantage in holding out against terms acceptable to oneself.

The account here summarised is developed with the aid of special concepts and terminology: the valuations, for example, are given by ophelimity functions in which the ophelimity is cardinal; a spokesman's estimation of the risk of a conflict is given by a correspection function which expresses his insight into the other man's outlook; and his weighing the benefits of settling now against trying for better terms and risking a conflict yields an expression called his propensity to fight. The functions enable the propositions about the behaviour of the spokesmen to be written as equations: thus the proposition that a spokesman will be ready to settle for the terms now open to him when he sets as high a valuation on them as on the combined possible outcomes of holding out, yields two final equations, one for each spokesman; and the task of the process of bargaining is seen as shifting the functions so that both equations can be satisfied by one and the same value of " terms now open ".

There may be a heuristic gain from postulating concepts not themselves open to observation, if they lend themselves to manipulations which afford fresh insights into the facts. But I have not myself found such insights here: the concepts and notation do not seem to add anything but complications to the argument as that can be set out in plain terms. When I come to assess it as such, I want to pay full tribute to the care and persistence with which it has been worked out, and to its instructive critique of some previous theories of bargaining, but I cannot say that it has added to my own understanding of what goes on at the bargaining table and what decides the outcome. It provides a formal framework into which every practical consideration can be fitted: the consideration, for instance, that a renewal of the recent stoppage in the steel industry of the United States would fall within the year of a Presidential election might be said to affect the ophelimity of conflict to either side or its correspection function of the outlook of the other. It is because he wants to be able to fit everything in that Professor Pen has eschewed case study. " Every consideration, every motivation, and every conceivable course of action of the bargainers will have to be incorporated in our solution . . . This aim of ours can only be achieved if our theory is of an instrumental nature. We shall not try to discover what in fact happened in various wage negotiations in the past, nor what might be expected to happen in the future. This would amount to detailed casuistry " (p. 17). But I feel that catch-all concepts lead to truisms rather than insights, and that we are likely to reach concepts that will organise our thought fruitfully only through observing " what in fact happened ".

For several reasons I have found the study of this book difficult. The exposition is prolix and runs much to taxonomy. Fig. 1, which is also reproduced as the design of the wrapper, requires drastic changes of lettering if it is to agree with the text, and amendments are also required in the lettering of Fig. 4, and in a textual reference to Fig. 2. The numerous notes oblige one to turn to the back of the book, except notes 28—43 on C.III, which I could not find anywhere. There is no index.

E. H. PHELPS BROWN.

The Economics of Underdevelopment. Edited by A. N. AGARWALA and S. P. SINGH. Oxford University Press. 1958. viii + 510 pp. 30s.

The post-war interest in problems of underdeveloped countries shows no signs of flagging. The last ten years have seen the appearance of scores of books, innumerable reports, both national and international, and a proliferation of journal articles on a scale which warrants yet another field of specialisation. At such a time the idea of bringing together in one volume some of the better known contributions for the convenience of the student, at home and abroad, is to be applauded. It may be said at the start that here is good value for one's money. The chapter from Viner's Brazilian lectures, the two conference papers, and the eighteen journal articles add up to a fair sample of the literature on this subject—though a contribution, say, from Bauer, Benham, or Hoselitz, would surely have been a gain in ideological coverage.

In as much as the level of sophistication among students to whom this book will appeal is bound to vary markedly, it is only a matter of precaution that things which are fairly obvious to the general run of economists should still qualify for some treatment. For instance, the idea that underemployment in poorer countries is not to be attributed to a lack of effective demand, and that " Keynesian economics " does not therefore apply, though not likely to be disputed can bear some emphasis. The same remark goes for the notion that in a capital-thirsty country, what little there is available should, ideally, be spread thin and even over the economy rather than agglomerate in a few chosen spots. Nevertheless, there are plenty of ideas here to capture the interest of the more mature though non-specialist reader.

The twenty-one papers are sorted out into six groups. The first deals with the general approach to underdeveloped countries (Viner, Colin Clark, Meier, Baran, Myint). The second discusses the historical context (Kuznets, Rostow). The third is on theoretical contexts (Wallich, Rao, Bruton). The fourth discusses external economies and balanced growth (Rosenstein-Rodan, Nurske, Fleming, Scitovsky, Furtado). The fifth deals with underemployment and factor-disequilibrium (Navarrete, Eckaus). The sixth offers us models of economic development (Singer, Lewis, Chenery, Bronfenbrenner). The names of many of these writers make comment superfluous.

In bringing these papers together the editors voice a hope that it will stimulate those in underdeveloped countries to think for themselves—an unexceptionable sentiment in itself. But in view of the context of the preceding remarks one has doubts about the moral intended. For, we are told, " it has been a standing criticism of the economics taught in underdeveloped countries that it is a carbon copy of the economics taught in and appropriate to industrially advanced countries ".

Now it is unlikely that a similar criticism would be levelled against the physical sciences. What, then, is particular to the method of economic science that its applicability should be so localised ? No reasons are given for this belief, and to infer it from a mere enumeration of differences in wealth, climate, institutions, or customs, is surely to misunderstand the nature of science in general, conceived as a search for uniformity amid diversity. True, the basic environment in which physical theories hold is granted to be much the same in any part of the globe, while one may be less certain of the relevant environment in regard to economic hypotheses. But one can argue till the Greek calends about whether some features are irrelevant parts of the heterogeneous universe in so far as particular theories are concerned, or whether, instead, they constitute a fundamental change in the economic environment which destroys the legitimacy of the theories in question, without ever being in sight of agreement. Such matters are not to be settled by eloquence, but by putting them to the test.

In the meantime, what *a priori* reasoning may be permitted would suggest that countries in which resources are scarcer than they are in the West, countries in which the populations live closer to subsistence, are quite fit subjects for traditional economic analysis. Certainly there is little analysis in this book that is not part of the accepted corpus of economic theory. A good deal of it—indeed, the best parts, in my opinion—spring from the central core of economics, the theory of value. Viner's lucid and scholarly essay is vindication enough for this view. Honourable mention should also go to Professor Eckaus for his masterly employment of the analysis of fixed factor proportions. His conclusions that limited capital equipment and fixed technical coefficients may aggravate underemployment, as will immobility and extravagant claims of militant trade unions, though apparently highly relevant to underdeveloped economies, will not affront the conscience of the most venerable " Classical ".

While agreeing that there is always scope for mischief in economics—more so perhaps than in the physical sciences—either because one does not appreciate the problem, because one shows poor judgment in choosing analytic tools, or because one makes no attempt to check one's conclusions, there is, as yet, no case for a different theory of economics peculiar to underdeveloped countries. Until the contrary is proved, students in underdeveloped countries who hope to contribute to the material advance of their countries may be safely urged to steep themselves thoroughly in the analytic techniques evolved in the West.

Finally, although this is a volume on *how* to develop and not *why* to develop, it is inevitable that in discussing the allocation of factors a good deal of welfare creeps in. Despite the fact that welfare economics is at present in a decidedly bruised condition, there is little to quibble at in such commonsense measures as moving superfluous labour from farms into industry, spreading information, teaching new techniques, and the like. To the more philosophical query, of whether one ought to industrialise at all, there is only the most perfunctory response. After all, the basic unwritten premise of economic science, it can be argued, is that economic development is a good thing. And though there are people today who are inclined to doubt it, for the West there does not appear to be any choice. For the West, in Rostow's aerial terminology, has " taken off ", and is willy nilly impelled into self-sustained growth. But for vast areas of Asia and Africa, where there is some choice since the process has not yet got under way, the almost desperate anxiety of an active minority to shake off the dust of centuries, to overcome the inertia of the masses, and to hustle in the new era, does make one wonder whether perspective has not been lost in the scramble for industrialization. It is not so much the pain of the initial wrenches at the roots of a traditional way of life that is at issue. We can presume that they, the minority at least, are prepared to make big sacrifices. But for what ? What vision do they have of things to come ?

Some features of the picture which is emerging are rather alarming. The most economically advanced country, the U.S., is apparently in the process of evolving a race of " organisation men ", who model their lives on the slogans of successful prototypes, repress all wasteful spontaneous impulses, and decide when to laugh on the principle of net advantages. They choose their club, recreations, holidays, community activities and friends solely with an eye to furthering their material prospects, expecting no less from their wives and children, and withal deeming such conduct virtuous and exemplary. Nor is this development a sociological aberration which can be overcome by television sermons; something which other countries may with diligence avoid. It is an inescapable concomitant of the pursuit of material ends, which pursuit to be effective requires that people be endlessly ambitious. The precondition of sustained growth is sustained discontent.

It is not suggested, of course, that all economists desirous of fostering economic growth should be requested to justify their attitude. But at a time when a large number of people in the West are voicing doubts about the effects on human happiness of the machine age in which we are caught up, it would be assuring if the editors of a volume on how to get there, or one of the writers at least, showed some awareness that, as present trends go, the thorny path to industrialisation leads, after all, only to the waste land of Subtopia. If they believe that somehow this ignoble fate can be circumvented, we should be told about it.

E. J. MISHAN.

Social Theories of Fertility and the Malthusian Debate. By D. E. C. EVERSLEY. The Clarendon Press, Oxford. 1959. ix + 313 pp. 35s.

Population has always been a subject likely to generate excitement, and no aspect of it more so than the attitudes taken toward fertility. Mr. Eversley's book is a judicious and scholarly account of the evolution of thought on this important theme. It is written with a succinctness and precision of phrase that give form to the extensive literature with which he has to deal, and which draw out the distinctive themes embodied in passages which, to the careless eye, might seem to be mere repetition. There is no *parti pris* attitude, and there are no polemics. We are reminded how, as the urge for close system seized classical economists, the more sociological aspects of economics were dropped, or frozen in the subsistence theory of wages. Just as the theory of aggregate demand sank to an " underword " of debate, so too did the sociology of fertility, once Nassau Senior and Richard Jones had completed the substitution of the psychological for the physiological interpretation. Men like Sadler, Doubleday, and the rest, received short shift from the pundits of political economy. But in this underworld were mulled many of the ideas that now find a place in the United Nations' *Determinants and Consequences of Population Trends.*

Indeed, Mr. Eversley, in dealing with theories of fertility, has encountered the problem of demarcation: how to avoid straying from current attitudes toward procreation, through work incentives, or savings, or employment, into the general theory of the economy. His early commentators might fling down their splendidly isolated *obiter dicta*, but as ideas became more numerous and more sophisticated, the problem of general interdependence slowly, almost imperceptibly, raised its head. But our author is aided in his task by the fact that the supersession of the Malthusian debate came in the form of other, but equally generalised, theories (the social biology of Spencer and the theories of concomitance of the statisticians)—methods of approach which make it possible to continue to do without a general theory of the economy.

The discussion of the view taken of luxury is an interesting example of the demarcation problem. Though luxury might be condemned, as Mr. Eversley shows, on fertility grounds, as causing sterility, many other elements were involved in the goodness or badness of luxury. It was well known that the lavishness of aristocrats and the wealthier gentry assisted regional and even national prosperity. This might well increase the fertility of the community as a whole, through the maintenance of incomes—circumstances leading to approval rather than condemnation of luxury.

Malthus is accused quite correctly (p. 255) of obstinacy in his refusal to give adequate weight to the action of preventive checks, and Mr. Eversley offers some conjectures as to why this was so, including the charge that Malthus was " a bad observer of his fellow human beings ". But surely the reason lies deeper than this. Malthus was caught between the need to amplify his theory, as demanded by scientific conscience, and the danger that if he did so he would weaken it as a basis for public policy and individual behaviour. It is this struggle that gives rise to the habit of which Mr. Eversley complains —making a substantive statement at one point, then making near-fatal concessions elsewhere.

A word or two about the influence of the psycho-analysts upon the fertility debate might have been interesting. In Malthus' mind the stresses engendered by the demand for continence were a means of promoting sound character and mental health, through self-discipline; the opposite view that such an experience could be severely detrimental to the mind appeared as early as the fifties in the works of George Drysdale (who is not mentioned in the present work in spite of his explosive effect upon Wicksell on these very grounds in the 1880's).

Mr. Eversley is careful to delineate the responsibility and authority of the historian in deriving lessons for contemporary guidance. The historical fact that, in western countries, as living standards have improved, so fertility has gone down, can, at best, " be regarded as a contribution towards the formulation of a theory ". But the historian here has a double duty: to make available not only such a generalised observation, but also to stress the precariousness of a situation in which both population and incomes are in an early stage of " take off "—the situation out of which the fears of Malthus arose. Mr. Eversley does not treat Malthus and his supporters as mere apologists for existing arrangements (though they were such, often in good conscience), but as men genuinely struggling to understand what was happening to their society.

It is easy enough to remark on the *simpliste* character of much that has been said in the past, and the various theories of single causation that gripped men's minds; but it is salutary to remember the complexity of the problem of isolating and understanding the demographic effects of a change in policy, or circumstance, and how tempting it still is to adopt similar drastic over-simplifications.

S. G. CHECKLAND.

Theory of Wages and Employment. By ALLAN M. CARTTER. Richard D. Irwin Inc., Homewood, Illinois. 1959. xii + 193 pp. $5.

The object of this study is to survey and reconsider the theory of the labour market, in the light of the changes in the structure of that market, and the developments of the analysis of imperfect competition and of aggregate income and employment, that have come about since J. R. Hicks' work on wages appeared in 1931, and Paul Douglas' in 1934.

The first part, it is true, goes farther back, to trace the rise of the marginal productivity theory through J. B. Clark and Marshall, but it goes on to examine the more recent discussion of the realism of the theory's assumptions, and apply it to the decisions of the firm in some typical conditions of cost and competition. There has been a notable lack of sources to which the student could be referred for an exposition sufficiently explicit and rigorous to do justice to the analysis on its own assumptions, yet also concerned to ask how far those assumptions hold. The clear exposition and careful argument of Professor Cartter's Part I go far to meet this need, and many teachers will be grateful for them.

The texture of Part II is different: its subjects are various, its statements compressed. Here first is a formalisation, developed through many graphical illustrations, of the economic objects of the trade union and the preferences of employers and unions in collective bargaining, and of bargaining power and bargaining attitudes; and an inquiry, in the light of Keynesian and later analysis, whether a general reduction in money wages will raise or lower total employment or leave it unchanged. There follows a chapter which examines the Cobb-Douglas function and Hicks' propositions as explanations of the factor distribution of national income, and finding that any such application of marginal productivity involves fallacies of composition, offers instead an original macro-economic model, in which investment depends on profits, and savings depend on the division of income between profits and pay, and this division must keep investment and savings equal; with statistical studies, also original, of the extent to which the stability of the share of labour arises from the changes in that share in particular sectors being offset by changes in those sectors' relative sizes, and of the trends of labour's share in the unionised and non-unionised sectors of the American economy. So much cannot be dealt with in short compass without the exposition sometimes presupposing previous acquaintance with the argument, and some propositions being stated shortly that are by no means self-evident; nor can the reader follow one inquiry far before he is lifted to the next. But touching on so much gives breadth to the survey, and the commentary is always active and pertinent.

E. H. Phelps Brown.

Statistics of Extremes. By E. J. Gumbel. Columbia University Press, New York. Oxford University Press. 1958. xx + 375 pp. £6.

It is still widely believed that statistical theory must always be essentially concerned with the behaviour of averages or similar composite quantities, that the statistician cannot cope with the exceptional, extreme members of a group. This book is entirely devoted to the statistical theory of " extreme " observations, which are the observations at or close to the largest and smallest in a sample. Extreme-value theory has considerable mathematical fascination, and contains some notable results, the most striking of which is worth mentioning here. As has been known for 30 years, no matter what the shape of the initial distribution from which we take a large sample, the distribution of the largest (or of the smallest) value observed must be one of three certain distributions; which of these three distributions holds in any particular case depends only on the general shape of the " tail " of the initial distribution. A good deal of this book is concerned with the detailed study of these three distributions and the relationships between them.

Apart from its mathematical interest, extreme-value theory has been successfully applied in many fields; indeed, the practical needs have often evoked the theory. Especially successful applications have been to the strength of structures and materials subject to occasional extreme stresses, and thus to problems of flood control; and to meteorological observations of many kinds, including problems of drought control and thence, by implication, to dam construction specifications. There is some discussion of the applications in this book and Professor Gumbel, who has devoted most of his working life to the theory and applications of extreme-values, is concerned to underline the practical lessons of the theory he derives (and occasionally even lovingly re-derives). But despite this and despite the extensive use of graphical and tabular presentation of theoretical results, this is essentially a book for the professional statistician. The less specialised reader would profit more from the same author's earlier pamphlet *Statistical Theory of Extreme Values and Some Practical Applications* (published in 1954 as No. 33 in the U.S. Government's National Bureau of Standards, Applied Mathematics Series) and even the intending specialist new to this field might take this pamphlet as a preliminary dose. The book contains an excellent and highly international bibliography, and an index quite inadequate for what is at present the only, and for many years will surely be the leading, reference book in this field.

Alan Stuart.

SHORTER NOTICES

Operations Research. An annotated bibliography. By JAMES H. BATCHELOR. St. Louis University Press. 1959. x + 866 pp.

This bibliography nominally covers all publications on operational research to the end of 1957. The entries are listed alphabetically by author, and the compiler has wisely made no attempt at classification. The layout in both the main section and in the excellent index (author, subject and title) is clear and legible, and full publication details are given. Most of the entries are annotated and many of them also record the whereabouts of reviews and comments. Despite some odd lacunæ in the coverage of work outside America, the selection procedure seems reasonably comprehensive and the book provides the most satisfactory bibliography on operational research yet published.

Business Enterprise in its Social Setting. By ARTHUR H. COLE. Harvard University Press. Oxford University Press. 1959. xiii + 285 pp. 45s.

Professor Cole attempts in this book a summary of findings arising from the work of the Harvard Research Center in Entrepreneurial History, of which he was for many years the head. He addresses himself primarily to those theoretical economists who write about *the* entrepreneur. If few of the conclusions presented here—above all, that economic history must take full account of innumerable non-economic factors, the " total cultural situation "—appear novel to the economic historian, this results in part from the important earlier work of Professor Cole and his collaborators. The main conclusion to be drawn from the generalizations about entrepreneurs contained in this book appears to be, however, that one cannot generalize about entrepreneurs; but that nevertheless, in Professor Cole's words, " entrepreneurial change is rather obviously an important phenomenon ".

The Shop Stewards' Movement and Workers' Control, 1910–1922. By BRANKO PRIBIĆEVIĆ. Basil Blackwell. 1959. xii + 179 pp. 25s.

Mr. Pribićević is a Yugoslav scholar who worked in Oxford under the supervision of G. D. H. Cole. This book presents his findings so far as they concern engineering. He does not so much describe what was going on in the shops, as narrate the history of two movements associated with the demand for workers' control, the Amalgamation Committee Movement from 1910 to 1917, and the Shop Stewards' and Workers' Committee Movement from 1915 to 1922, and trace the various and changing connections between these movements and syndicalism, industrial unionism, guild socialism, and subsequently communism. His work provides an instructive account, in much detail not previously available, of a significant phase of British industrial relations. In a noteworthy Foreword, which must be among the very last of his writings, G. D. H. Cole reflects on his own thought and activities in those years.

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Fluctuations in Trade, Industry, and Finance

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